PHILOSOPHICAL

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the Year 1800, the Medal on Sir GODFREY COPLEY's Donation, to EDWARD HOWARD, Esq. for his Paper on a new fulminating Mercury.

PHILOSOPHICAL

TRANSACTIONS.

I. *The Croonian Lecture. On the Irritability of Nerves.* By Everard Home, *Esq. F. R. S.*

Read November 20, 1800.

THE nerves have been hitherto considered as chords that have no powers of contraction within themselves, but only serving as a medium, by means of which the influence of the brain may be communicated to the muscles, and the impressions made upon different parts of the body conveyed to the brain.

The difficulties which attend every attempt to investigate the real state of the nerves in the living body, and the impossibility of acquiring any information upon this subject after death, may be urged in excuse for this opinion having been so universally received, since it will be found, from the following experiments and observations, to be void of foundation.

The only means by which any knowledge respecting the irritability of nerves can be procured, must be from the operations in surgery performed upon nerves, either in a healthy state, or under the influence of disease; or from experiments

made upon animal bodies before they are wholly deprived of life, and instituted for that particular purpose.

My attention was directed to this subject by the following case, which explains many circumstances respecting the actions of the nerves when under the influence of disease, and gave rise to the experiments and observations contained in this Paper.

A person thirty-six years of age, naturally eager and anxious in his disposition, whose stomach was peculiarly irritable and irregular in its action, in the winter of the year 1796, while riding in the country, was thrown from his seat by a sudden motion of the horse; and, in endeavouring to save himself, fell with his whole weight upon the end of his thumb, against the pommel of the saddle.

The part swelled, and became very painful. A few days after, he hurt it again, which prevented the swelling from subsiding, and it remained uneasy and enlarged for three or four months. It afterwards got well, but the motions of the thumb were not always under the command of the will; so that he was sensible, in the years 1797 and 1798, while writing, of finding a difficulty in forming particular letters.

On the evening of the 16th of October, 1799, which was cold and damp, he was travelling in a post-chaise with two other persons, and let down the window, to speak to the driver. A cold wind blew directly into the carriage, and he endeavoured to pull up the window; but, not seeing the glass rise, he looked down, and his hand, instead of pulling up the window, was lying upon his knee. The thumb was bent in towards the palm of the hand; a spasm came upon the muscles of the arm, making them bend the elbow; and immediately he became insensible: in a quarter of an hour he perfectly recovered himself. Some

hours after, upon bending his thumb, to shew what had happened to him in the carriage, there was a return of the same attack, which also rendered him insensible for a few minutes.

From this time, he had no return of these attacks for nine weeks; at the end of which period, on the 18th of December, 1799, he was waving his hand over his head, with a degree of eagerness, as a sign for some people to make haste and follow him; this exertion made the thumb contract towards the palm of the hand, and he fell upon the ground in a state of insensibility. This attack went off as the others had done; he had another in the evening; and, in the course of the next day, two more, equally violent. As the motion of the thumb was the first symptom in all these attacks, the assistants were led to contrive a glove, the front of which was strong enough to resist the motion of the thumb, and to keep it in its place: while this was kept on, the attacks were less frequent. A ligature was then applied round the fore-arm; when the thumb was beginning to be agitated, this was tightened, and the spasms were found to be arrested at the ligature, and of course deprived of their violence.

From this time, a tourniquet was kept constantly upon the fore-arm; and a person was always in readiness to tighten it, the moment the spasm was expected, which was always preceded by a general feel of uneasiness all over the body: as soon as the spasm went off, which it did instantaneously, the tourniquet was loosened. The spasms in the thumb and fore-arm returned frequently, and at irregular intervals, generally every three hours, sometimes oftener, and once did not come on for thirty-six hours.

On the third or fourth day, electricity was tried, with a view

to relieve them; sparks drawn from the thumb, produced tremors in the muscles, which were confined to the thumb. An electric shock through the ball of the thumb, brought on a very severe spasm in the arm; but neither sparks, nor a shock through the other thumb, produced any sensible effect.

On the 29th of December, I first saw the patient; and, after watching the symptoms for three days, made the following observations upon the complaint.

That the beginning of the attack was some involuntary motion of the thumb and fore-finger; and therefore, the disease appeared to be in the branch of the nerve which supplies these two parts, called by WINSLOW, the median nerve.

That the progress of the spasms was in the direct course of the trunks of the median nerve, up to the head.

That compressing the parts in the course of that nerve, when it was done before the spasms had reached them, always arrested their progress; but, when once the muscles had become convulsed, or agitated, the same compression had no effect in stopping the progress of the spasms.

The mode in which the spasms were propagated along the course of the nerves, was as follows.

Five or six tremors took place in the flexors of the thumb and fore-finger; then similar convulsive motions affected the muscles of the fore-arm; soon after, the muscles of the arm were thrown into the same kind of action; afterwards the pectoral muscle, and scaleni of the neck: the muscles of the lower jaw were probably in the same state, although their action was not within the notice of the by-standers. The head was pulled forcibly to that side, in quick successive motions, and in a second or two, the whole ceased; the parts became tranquil, the insen-

sibility went off, and the patient recovered himself: there was, however, a general feel of languor and distress over the whole body, before the recovery.

From these observations, the disease appeared to be decidedly in the inferior branches of the median nerve; and the irritation was conveyed along its course, from its terminations in the thumb and fore-finger, to the origin in the brain.

It was proposed to divide the nerve, as it passes from under the annular ligament of the wrist towards the thumb, to cut off the communication between the diseased extremities and the trunk of the nerve, and so put a stop to the progress of the irritation which constituted the disease.

That such an operation might be attended with success, was not only rendered probable from reasoning, but the performing it was fully justified by the success which had been experienced from a similar operation, in some cases of the *Tic douleureux;* a disease, in many respects, of the same nature with the present.

All these circumstances were explained to the patient, who, from a desire of obtaining relief, consented to have the nerve divided. This was done on the 1st of January, 1800, in the following manner: the nerve, as it passes from under the annular ligament, towards the thumb and fore-finger, was laid bare, for above an inch in length; it was then detached from its lateral connections, and, in this exposed state, a probe-pointed bistoury was passed behind it, and the nerve was raised upon the edge of the instrument, so as to be distinctly seen by the different medical gentlemen present, before it was cut through. As soon as it was divided, the two cut ends retracted from one another, to a considerable distance. This retraction was very unexpected, as the nerve was disengaged from the cellular membrane, and

no other part had been divided, whose action could make the portions of the nerve recede.

That nerves, when divided, do retract, is well known in the practice of surgery; but this effect has been usually attributed to the contraction of the neighbouring parts, as the cellular membrane and blood-vessels, with which the nerves are connected. As none of these causes could produce the effect in the present instance, it was natural to suppose, that an independant action existed in the nerve itself, which had been so much increased by the influence of disease, as to become unusually great; and, therefore, the retraction was more distinctly seen than in a healthy state of the body.

The moment the nerve was divided, there was a spasm over the whole body, and a momentary insensibility. The blood-vessels divided in the operation were not secured by ligature, but allowed to stop of themselves, to give the wound every chance of healing by the first intention. The edges of the skin were carefully brought together, and kept in that state by compress and bandage, to promote as much as possible the union.

For eight hours after the operation, the parts were perfectly quiet, and there was no spasm. The wound then began to feel hot, as if a red hot coal had been applied to it. To relieve this sensation, the outer bandage was loosened, and immediately there were twitches in the nerve, which soon went off. The patient felt himself generally unwell, extremely nervous, and irritable.

Fifteen hours after the operation, he had a violent spasm, which went along the arm to the head, but did not affect the brain. In an hour, there was a second attack, at which I was

present; the pulse was 105 in a minute, the tongue white, a great deal of general irritation, nervous twitches all over the body, but in the greatest degree in the arm and leg of that side. The stiff-fronted glove was now put on, to confine the thumb.

Twenty-four hours, or one day, after the operation, the first dressings were removed: the thumb was much swelled, and no union whatever had taken place; the spasms returned every five hours, but were less violent.

The second day, there was no abatement of the symptoms, but the spasms did not affect the brain; they were not now stopt by the pressure of the tourniquet, as they had been before the operation.

The third day, there were intervals of ten hours between the spasms; and, in the night, they did not extend beyond the elbow.

The fifth day, suppuration took place in the wound; the swelling in the hand was much abated; and the patient was able to dress and shave without spasm, having only twitches in the fingers, and tremors in the fore-arm.

The sixth day, there was a burning pain in the hand, and a numbed heavy feel in the thumb and fore-finger, similar to what the patient recollected to have felt four years before, when he hurt his thumb.

The seventh day, the patient awoke with great pain in the hand, succeeded by a violent spasm, which passed up to the head, although the tourniquet had been previously tightened: after this, he had no spasm for sixteen hours.

The eighth day, the hand was less swoln and less painful; and he had only two spasms in twenty-four hours.

The ninth day, the swelling had subsided, and the twitches

ceased; in thirty hours, there was only one slight spasm, which did not go beyond the wrist.

The sixteenth day, the wound was intirely healed; and, as there had been no return of spasms, the patient was considered as well.

On the twenty-fourth day, which was a fortnight after the spasms had ceased, at nine o'clock in the morning, he was awakened by a violent spasm, which passed directly up to the head, and affected the brain, producing insensibility; this was the only time the brain had been affected since the operation.

Two days previous to this attack, he had a violent diarrhœa; and, on the preceding day, had undergone unusual fatigue.

The tourniquet, which had been laid aside, was now applied; and, for the greater security, two were placed on the fore-arm, and one upon the arm itself. At six in the evening, there was another spasm, attended by insensibility, although the tourniquets had been tightened. The hand was found swelled, as well as the wrist, and the cicatrix formed a hard welt, tender to the touch. This hard state of the cicatrix, in which the end of the divided nerve was included, appeared to be a probable cause of the return of the spasmodic attacks.

The twenty-fifth day, the pulse was 100 in a minute; and, every two hours, there were slight spasms.

The twenty-sixth day, there were eleven spasms, at irregular intervals, in twenty-four hours, eight of which, went up as high as the head. As the spasms were not stopped by the tourniquet, as before, it was proposed to make the pressure directly upon the nerve: this was done by placing pieces of cork in the course of the nerve, and confining them there by the band of the tourniquet, so that, when the screw was tightened, the cork was

pressed down on the nerve. This pressure gave great pain, and, instead of arresting the progress of the spasms, seemed rather to increase their violence; it was therefore left off.

The twenty-seventh day, the pulse was only between 80 and 90 in a minute; there were seven spasms, all of which were arrested by the first or second tourniquet.

The spasms went on, with very little variation, till the 39th day at six o'clock in the morning, when he was seized in his sleep with a violent spasm, attended with insensibility, and convulsions over the whole body: these lasted for twenty minutes. After his recovery, the hand was found much swoln, and the welt formed by the cicatrix was painful. In the course of the forenoon he was well enough to bear going out in the carriage; the fresh air always proving very grateful to him.

From this time, the swelling of the hand and the hardness of the welt diminished; and the spasms were less violent, and seldomer. On the 45th day, there was only one slight spasm in twenty-six hours. In this state he went into the country; and, for the first fortnight, the spasms diminished, but afterwards became more violent.

The return of the spasms after the wound had been healed, made it evident, that the operation of dividing the nerve had not answered the purpose which was expected from it. The failure probably arose from the wound not healing by the first intention: the consequent inflammation rendered the cut end of the nerve uncommonly irritable; and, in this state, the confinement in the hard thickened cicatrix, rendered it liable to be stretched by every motion of the thumb, so as to bring on spasmodic contractions.

From this time, the patient was not under my direction; but

I understood, that he tried the effect of large doses of opium, which did not afford relief. He was then induced to employ electricity, which was also unsuccessful; and he died in a fit, which at the time was believed to be apoplexy, about five months after the operation had been performed; but, as the body was not examined, the nature of the fit could not be ascertained.

In this case, some of the branches of the median nerve had acquired, from disease, an unnatural power of contraction, which was made evident by the operation; and there is every reason to believe, that the spasmodic attacks which took place, were in reality convulsive motions in the nerves themselves, which excited corresponding contractions in those muscles that were under their influence.

This case naturally occupied my mind; and I could not avoid dwelling upon many of the extraordinary symptoms which made a part of it; but nothing so impressed itself upon me, as the retraction that took place in the cut ends of the nerve, at the time of the operation.

The first idea which suggested itself was, to endeavour to ascertain whether this retraction arose from an increase of a natural action in the nerve, or from one newly acquired, produced by disease.

With a view to ascertain this point, different experiments were instituted. The object of these was, to determine whether a similar contraction took place in nerves, when divided in a healthy state of the body; the extent of such contraction, if any occurred; and the circumstances by which it may be influenced.

For the first of these purposes, the following experiments were made.

Exper. 1. The cutaneus internus nerve of the fore leg of a young rabbit was laid bare, where it passes down before the biceps flexor cubiti muscle: the nerve was disengaged from its lateral attachments; and, while the limb was in a moderately extended state, a probe-pointed bistoury was passed behind it, by which means it was divided transversely. The two ends immediately receded from each other: the upper portion appeared to retract more than the other, and the end lay close to the muscle, in a straight line, while the end of the lower portion was a little bent to one side. The space between them, when measured by a pair of compasses, was found to be $\frac{2}{8}$ of an inch.

The branch of the musculo-cutaneus nerve, which lies near to the cutaneus internus, was divided in the same manner; and the retraction of the cut ends was found to have been to the same extent.

In this experiment, the limb was extended, although by no means to its utmost limits; it therefore became a question, whether the same degree of retraction would take place in the bent state of the limb.

To determine this point, the experiment was repeated, after an interval of four days, upon the other fore leg of the same rabbit, with the limb in the bent state: the retraction, however, was found to have been exactly to the extent of $\frac{2}{8}$ of an inch.

From this experiment, made under these different circumstances, a retraction of the cut ends of a divided nerve was ascertained to take place, which led to the further prosecution of the inquiry.

For this purpose, the phrenic nerve in the horse was selected, as being more favourable, in many respects, than most others

in the body, both from its superficial situation in the chest, and its great extent without giving off any branches.

In making experiments of this nature, it is an advantage that the animal should be of a large size; and the mode in which horses are killed in London, affords an opportunity of experiments being made on that animal, without giving the operator the painful sensations of having made any addition to its sufferings.

As horses are killed at stated times only, and these occur in a part of the day which is necessarily occupied by my professional engagements, the following experiments were made by Mr. CLIFT, the Conservator of the Hunterian Museum, whose accuracy may be relied upon, as well as his abilities in conducting them, having been early initiated, and long experienced, in inquiries of this nature.

Exper. 2. Immediately upon a horse having been knocked down, the thorax was laid open, and the phrenic nerve of the right side, passing round the pericardium, was exposed. It was nearly of the size of a crow-quill, and slightly connected with the pericardium. In this state, the point of one blade of a pair of scissars was passed under the nerve; and, by closing them, the nerve was transversely divided, without the smallest disturbance to its lateral connections. The two cut ends immediately retracted from each other, leaving the space of one inch between their extremities.

This experiment was repeated upon a second horse; and the retraction of the cut ends of the nerve was found to be exactly one inch.

It was repeated upon a third horse; and the retraction was found to be nearly two inches. In measuring the space between

the two ends of the nerve, the compasses accidentally touched the lower portion, and the diaphragm was immediately thrown into action.

The result of this experiment, not only confirmed the former, which had been made upon the rabbit, but it proved in the most satisfactory manner, that any action the nerves are capable of exciting, is nearly as strong after apparent death has taken place from a violence committed upon the brain, as while the animal is in perfect health.

Monsieur PORTAL, in a paper on a new mode of performing the operation of amputation, published in the Memoirs of the Academy of Sciences for the year 1773, mentions an experiment made on the sciatic nerve of a dog, in proof of nerves not having a power of retraction, at least none deserving of notice.*

This experiment was repeated by Mr. CLIFT, on the sciatic nerve of a rabbit. Immediately on dividing the nerve, the cut ends receded from one another: but, that the result might be exactly ascertained, the rabbit was killed half an hour after the experiment was made; the parts were carefully dissected, and the space between the two cut ends measured; which was exactly $\frac{6}{10}$ of an inch.

To ascertain whether this retraction was the consequence of a change taking place in the nerve itself, or arose from any other cause, the following experiment was made.

Exper. 3. As soon as a horse was knocked down, the chest was laid open, and the phrenic nerve of the right side was exposed: twelve inches in length were immediately measured by a pair

* Memoire sur une nouvelle methode de pratiquer l'Amputation des Extrémités, par M. PORTAL. *Histoire de l'Academie des Sciences*, 1773. p. 542.

of compasses; and the limits of this portion were marked by a small pin, passed transversely through the substance of the nerve. The part included between the two pins was then separated from the rest of the nerve, in the following manner. The person who was to divide the nerve had a pair of scissars in each hand; and, having passed the point of one of the blades under the nerve, above the upper pin, and having done the same with a blade of the other pair of scissars, below the lower pin, the two pair of scissars were shut at the same moment, and the nerve at these two parts cut through.

This portion was again measured, and, instead of being twelve inches, was now only eleven and $\frac{1}{8}$; so that the irritation produced by dividing it, had made it contract $\frac{7}{8}$ of an inch.

This experiment was repeated upon several horses; and in all of these repetitions there was a contraction produced: this varied in the different experiments, and in some of them was only $\frac{3}{8}$ths of an inch. When the nerve was divided very early after the animal had been knocked down, it was the greatest; and, in proportion to the delay that took place, so was the diminution in the degree of the contraction.

In these experiments, the nerve, as well as the surrounding parts, was disturbed as little as possible, that the results might be the more readily and more accurately ascertained: this, however, makes them liable to an objection, which is, that the contraction might be produced by the cellular membrane surrounding the nerve; an objection which certainly can have little weight in the peculiar situation of the phrenic nerve, as it lies between the pleura and pericardium, where the cellular membrane can have little influence over it, while the pericardium is left entire.

As, however, the opinion of the cellular membrane being the agent by which the retraction of divided nerves is produced, has been very generally received, it was highly proper to attend to that circumstance, and have the experiment made in such a way as to prevent any other surrounding part from acting upon the nerve; with this view, the following experiment was made.

Exper. 4. The pleura was removed from twelve inches of the phrenic nerve of a horse; and afterwards the attachments between the nerve and pericardium were completely divided: under these circumstances, this portion of nerve was separated, as in the last experiment. This portion was again measured, three hours after, in its detached state, and it was found to have lost $\frac{6}{8}$ths of an inch in length. The horse was twenty years old, and was killed on account of its age, which rendered it by no means a favourable subject for such an experiment.

With a view to determine whether the power of contraction in a nerve continued for any length of time after apparent death had taken place, and also to ascertain what proportion of elasticity a nerve possesses, (for every part of an animal body that is not rigid, appears to be endowed with it in a greater or less degree,) the following experiment was made.

Exper. 5. Eighteen inches in length of the phrenic nerve were measured, and separated by means of scissars: the contraction produced was only $\frac{3}{8}$ of an inch; the experiment being made nearly an hour after the horse was knocked down. Upon being stretched with force, it elongated to $18\frac{1}{2}$ inches; and, on being left to itself, retracted to $17\frac{7}{8}$. It was kept till next day, and again measured, when it was only $17\frac{5}{8}$: upon being stretched, it was elongated to $18\frac{1}{2}$; but, immediately on being left to itself, it retracted to eighteen inches.

This experiment was repeated upon another horse; and the result was similar, both with respect to the contraction which took place after the nerve had been removed from the body, and the elongation which depended upon elasticity.

To ascertain if there was any difference in the appearance of a nerve when contracted, from one in a relaxed state, the following comparison was made.

Exper. 6. A portion of the phrenic nerve, about eight inches long, was removed immediately after the horse had been knocked down. This was allowed to contract; and, after it had remained quiet for twenty-four hours, its external surface was exposed by dissection, so that the appearance of its fibres could be distinctly seen. A portion of the same length was removed from another horse who died a natural death, and these were compared together.

The difference in the appearance of these two portions was very great: in the contracted nerve, the fibres were all serpentine; in the other, they were straight. The annexed plate, (see Plate I.) in which they are represented, shows very correctly, the great contrast which they exhibited.

The serpentine transverse lines described by MONRO, appear to be an effect of this contraction of the nerve; as they disappear when the nerve is relaxed or elongated.* These serpentine lines in the phrenic nerve, in a man who died of a locked jaw, when examined twenty-four hours after death, were much more distinct and regular than in the phrenic nerve of a man who died of a mortification of his arm.

* "When the nerve is fully relaxed, these serpentine transverse lines are best seen; when the nerve is moderately stretched, they are much less evident; when the nerve is greatly stretched, beyond what it ever is in a living sound animal, it appears

These experiments, upon so large an animal as the horse, made by a person well qualified for the purpose, and repeated sufficiently often to preclude any material fallacy, admit of the following conclusions being drawn from them.

1. That the nerves of an animal in health are capable of retracting themselves when divided; and that this effect is intirely independent of the parts by which they are surrounded.

2. That this contraction takes place in the nervous fibres themselves; and is independent of the brain, from which they originate, and of the muscles and other parts in which they terminate.

3. That the contracted nerve exhibits to the eye an appearance of contraction in its fibres, not to be seen when it is in a relaxed state.

As the nerves are so readily influenced by electricity, in exciting the muscles to action, it naturally suggested itself, that some further information might be obtained in the present investigation, by means of experiments made upon the nerves by the electric fluid. With this view, the following experiments were instituted; and Mr. CARPUE very obligingly assisted Mr. CLIFT in making them, and carried one of Mr. CUTHBERTSON's large plate-glass electrical machines to the slaughter-house, for that purpose.

Exper. 7. A portion of the phrenic nerve, twelve inches long, was exposed, and divided at both ends, as in the former expe-

uniform in its colour and consistence.—Hence these lines, are in the *first* place, to be considered as folds or joints in the nerve, and may be compared to the lines in the palm of the hand, serving to accommodate the nerve to the different states of flexion and extension."——(In a note,) "By soaking in water, this appearance is lost." MONRO *on the Nervous System*, p. 39.

riments. When it had contracted to 11 $\frac{1}{8}$, a strong electric shock was passed along its substance, from one end to the other; but, when measured again, the length was exactly the same. The portion of nerve was then dissected out, and laid upon a piece of glass; in its detached state, it measured 11 $\frac{5}{8}$. Several strong electric shocks were passed through it, in the direction of its fibres; but they did not produce the smallest effect upon it.

This experiment was repeated upon another horse, and the result was the same.

Exper. 8. Half an hour after a horse had been knocked down, 24 inches in length of the nerve called par vagum were laid bare, and a portion of it detached from its lateral connections, so that a piece of glass, 12 inches long, was admitted under it, without dividing the nerve from the trunk; in this state, electric sparks were drawn from it, and several strong electric shocks passed through it; but there was not the smallest change to be perceived, either in its length or appearance.

From these experiments it appeared, that when the nerve had contracted itself, in consequence of being divided, no increase of that contraction was produced by the electric fluid.

To ascertain whether electricity was capable of exciting contraction in a nerve that had not been previously irritated, the following experiment was made.

Exper. 9. Twelve inches of the phrenic nerve were measured; and the limits of that portion marked, by pins stuck through the nerve. This portion of nerve, in its relaxed undisturbed state, had electric shocks passed along its substance; but these were found, upon measuring the portion of nerve, to have produced no contraction in its length. When this portion was

separated, as in the former experiments, it contracted to 11 $\frac{3}{8}$ inches; a diminution of $\frac{5}{8}$ of an inch.

The electric fluid, in this last experiment, excited the action of the diaphragm, but produced no evident or permanent contraction of the nerve; and, when the nature of the contraction of a nerve is considered, it is not to be expected that permanent contraction can be ascertained, in any other way than by separating intirely a portion of nerve from the rest of the system. For the action is continued in tremors along the nerve, in quick succession; and, when the muscle has been excited to contract, the complete action of the nerve is finished, and it immediately relaxes, or returns to that state which admits of a new action.

This appeared to be the case in several experiments made upon the nerves of frogs, and of quadrupeds of a higher order, by two different metals, as described by GALVANI. In all of them, there was a convulsion of the muscle, and a tremor in the nerve; but, such was the rapidity of the effect, that it could not be decided that any motion took place in the nerve, except what arose from the agitation produced by the action of the muscle.

The experiments and observations which have been related, appear to illustrate an action in the nervous chords, capable of producing the symptoms which occurred in the case related in the former part of this paper, and also those met with in many other diseases, the symptoms of which have never been satisfactorily explained.

The hypothesis of a nervous fluid, although it may explain every symptom which originates in the brain, and from thence pervades any part of the system, and every symptom which

begins in the extreme parts and goes to the brain, does not give a satisfactory solution of those nervous agitations brought upon an extreme part, which only proceed for some way in the course of a nerve, and are there arrested, without being allowed to proceed to the brain.

The circumstance of nerves having been divided, and their functions being restored twelve or twenty-four months after, when the two cut ends have been united by a new substance, is a strong argument against the circulation of a nervous fluid; since no such effect takes place in the pervious canals of the body.

In many diseases, there are symptoms so decidedly confined to the course of the nervous chords, that an impartial observer would be unable to account for them, in any other way than by supposing them to arise from some action in the nerves themselves.

This idea must have been strongly impressed upon the mind of Dr. MEAD, who, in treating of his third sort of Quincy, says, all the nerves are convulsed, and the patient drops down dead suddenly.*

The *Tic douleureux* is a remarkable instance of this kind, both in the circumstances under which the spasmodic tremors are brought on, and the manner in which they are propagated along the nerve.

In one case of this disease, in which the operation of dividing the nerve was performed, with a view to remove the complaint, union by the first intention did not take place; and, during the time the wound was open, the inflamed state of the cut end of the nerve, made the patient liable to several attacks of the disease, similar to those he experienced before the operation;

* MEAD's Præcepta Medica. Quarto, p. 434.

but there was no recurrence of them after the wound was completely healed.

This is a very important fact; as it proves that inflammation on the cut end of a nerve, while in an irritable state, is capable of producing exactly the same symptoms as the original disease. This effect of inflammation upon the end of a nerve, explains the startings of the limb which occur too frequently after amputation.

These most commonly are met with when the limb is taken off above the knee, and the nerves and vessels have been previously inflamed higher than the part at which they were divided; and where the nerve is confined by the thickened state of the surrounding parts.

The same fact also explains the cause of locked jaw, when it is produced by a wound or bruise upon a nerve, in a constitution either rendered irritable by climate, or naturally so; also where the nerve itself becomes diseased, in consequence of the accident.

The following case of locked jaw, from an injury to the thumb, bears so great a resemblance to the case related in the beginning of this paper, as to show that the diseases must be nearly allied.

A lady of a very irritable habit was overturned in her carriage, and hurt her thumb, which swelled very much; and the skin over the metacarpal bone of the fore-finger, about the size of a shilling, sloughed off. No symptoms came on for fourteen days after the accident, when, upon bending her fingers, violent spasms took place in the thumb, which proceeded up to the neck and lower jaw; these were exceedingly painful, and the jaw was so much shut as hardly to admit a tea-spoon. In

fourteen days more, the jaw began to open; and, for a month longer, there were only two or three spasms daily in the thumb, attended with pain; these went up the arm to the jaw. At the end of that period, the sore on the back of the hand healed, and she recovered perfectly from the spasmodic affections.

To enter further into the histories of cases which afford evidence of a morbid action in the nerves, would be trespassing too far upon this learned Society, and would render the present Paper an inquiry into medical facts, which is only intended to be an investigation of the natural actions of the nervous fibres, illustrated by the phænomena which occur while these chords are under the influence of disease.

EXPLANATION OF THE FIGURES. (See Plate I.)

Fig. 1. A portion of the phrenic nerve of the horse, as it appears when in a contracted state.

Fig. 2. The same, as it is seen when magnified.

Fig. 3. A portion of the phrenic nerve of the horse, as it appears when elongated, or in the state of complete relaxation.

Fig. 1.

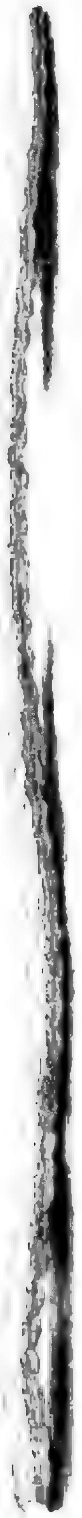


Fig. 2.

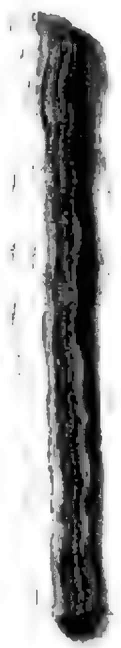
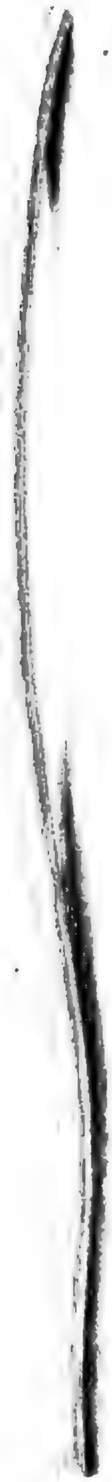


Fig. 3.



Basire sc.

II. *The Bakerian Lecture. On the Mechanism of the Eye.* By Thomas Young, *M. D. F. R. S.*

Read November 27, 1800.

I. In the year 1793, I had the honour of laying before the Royal Society, some observations on the faculty by which the eye accommodates itself to the perception of objects at different distances.* The opinion which I then entertained, although it had never been placed exactly in the same light, was neither so new, nor so much forgotten, as was supposed by myself, and by most of those with whom I had any intercourse on the subject. Mr. Hunter, who had long before formed a similar opinion, was still less aware of having been anticipated in it, and was engaged, at the time of his death, in an investigation of the facts relative to it; † an investigation for which, as far as physiology was concerned, he was undoubtedly well qualified. Mr. Home, with the assistance of Mr. Ramsden, whose recent loss this Society cannot but lament, continued the inquiry which Mr. Hunter had begun; and the results of his experiments appeared very satisfactorily to confute the hypothesis of the muscularity of the crystalline lens. ‡ I therefore thought it incumbent on me, to take the earliest opportunity of testifying my persuasion of the justice of Mr. Home's conclusions, which I accordingly mentioned in a Dissertation published at

* Phil. Trans. for 1793, p. 169.

† Phil. Trans. for 1794, p. 21.

‡ Phil. Trans. for 1795, p. 1.

Gottingen in 1796,* and also in an Essay presented last year to this Society.† About three months ago, I was induced to resume the subject, by perusing Dr. PORTERFIELD's paper on the internal motions of the eye;‡ and I have very unexpectedly made some observations, which I think I may venture to say, appear to be finally conclusive in favour of my former opinion, as far as that opinion attributed to the lens a power of changing its figure. At the same time, I must remark, that every person who has been engaged in experiments of this nature, will be aware of the extreme delicacy and precaution requisite, both in conducting them, and in drawing inferences from them; and will also readily allow, that no apology is necessary for the fallacies which have misled many others, as well as myself, in the application of those experiments to optical and physiological determinations.

II. Besides the inquiry respecting the accommodation of the eye to different distances, I shall have occasion to notice some other particulars relative to its functions; and I shall begin with a general consideration of the sense of vision. I shall then enumerate some dioptrical propositions subservient to my purposes, and describe an instrument for readily ascertaining the focal distance of the eye. On these foundations, I shall investigate the dimensions and refractive powers of the human eye in its quiescent state; and the form and magnitude of the picture which is delineated on the retina. I shall next inquire, how great are the changes which the eye admits, and what degree of alteration in its proportions will be necessary for these changes, on the various suppositions that are principally

* De Corporis humani Viribus conservatricibus, p. 68.

† Phil. Trans. f o, p. 146. ‡ Edinb. M l. Essays, Vol. IV. p. 124.

deserving of comparison. I shall proceed to relate a variety of experiments which appear to be the most proper to decide on the truth of each of these suppositions, and to examine such arguments as have been brought forwards, against the opinion which I shall endeavour to maintain; and I shall conclude with some anatomical illustrations of the capacity of the organs of various classes of animals, for the functions attributed to them.

III. Of all the external senses, the eye is generally supposed to be by far the best understood; yet so complicated and so diversified are its powers, that many of them have been hitherto uninvestigated; and on others, much laborious research has been spent in vain. It cannot indeed be denied, that we are capable of explaining the use and operation of its different parts, in a far more satisfactory and interesting manner than those of the ear, which is the only organ that can be strictly compared with it; since, in smelling, tasting, and feeling, the objects to be examined come almost unprepared into immediate contact with the extremities of the nerves; and the only difficulty is, in conceiving the nature of the effect produced by them, and its communication to the sensorium. But the eye and the ear are merely preparatory organs, calculated for transmitting the impressions of light and sound to the retina, and to the termination of the soft auditory nerve. In the eye, light is conveyed to the retina, without any change of the nature of its propagation: in the ear, it is very probable, that instead of the successive motion of different parts of the same elastic medium, the small bones transmit the vibrations of sound, as passive inelastic hard bodies, obeying the motions of the air in their whole extent at the same instant. In the eye, we judge very precisely of the direction of

light, from the part of the retina on which it impinges: in the ear, we have no other criterion than the slight difference of motion in the small bones, according to the part of the tympanum on which the sound, concentrated by different reflections, first strikes; hence, the idea of direction is necessarily very indistinct, and there is no reason to suppose, that different parts of the auditory nerve are exclusively affected by sounds in different directions. Each sensitive point of the retina is capable of receiving distinct impressions, as well of the colour as of the strength of light; but it is not absolutely certain, that every part of the auditory nerve is capable of receiving the impression of each of the much greater diversity of tones that we can distinguish; although it is extremely probable, that all the different parts of the surface exposed to the fluid of the vestibule, are more or less affected by every sound, but in different degrees and succession, according to the direction and quality of the vibration. Whether or no, strictly speaking, we can hear two sounds, or see two objects, in the same instant, cannot easily be determined; but it is sufficient, that we can do both, without the intervention of any interval of time perceptible to the mind; and indeed we could form no idea of magnitude, without a comparative, and therefore nearly cotemporary perception of two or more parts of the same object. The extent of the field of perfect vision for each position of the eye, is certainly not very great; but it will appear hereafter, that its refractive powers are calculated to take in a moderately distinct view of a whole hemisphere: the sense of hearing is equally perfect in almost every direction.

IV. DIOPTRICAL PROPOSITIONS.

Proposition I. *Phenomenon.*

In all refractions, the ratio of the sine of the angle of incidence to the sine of the angle of refraction is constant. (NEWTON's Opt. I. Ax. 5. SMITH's Opt. 13. WOOD's Opt. 24.)

Scholium 1. We shall call it the ratio of m to $m \mp 1$, and $m \mp 1$, n. In refractions out of air into water, $m = 4$ and $n = 3$, very nearly; out of air into glass, the ratio is nearly that of 3 to 2.

Scholium 2. According to BARROW, (*Lect. Opt.* ii. 4.) HUYGENS, EULER, (*Conject. phys. circa prop. soni et luminis. Opusc. t. ii.*) and the opinion which I lately submitted to the Royal Society, (Phil. Trans. for 1800. p. 128,) the velocity of light is the greater the rarer the medium: according to NEWTON, (Schol. Prop. 96. l. i. Princip. Prop. 10. p. 3. l. ii. Opt.) and the doctrine more generally received, the reverse. On both suppositions, it is always the same in the same medium, and varies in the ratio of the sines of the angles. This circumstance is of use in facilitating the computation of some very complicated refractions.

Proposition II. *Phenomenon.*

If between two refracting mediums, a third medium, terminated by parallel surfaces, be interposed, the whole refraction will remain unchanged. (NEWTON's Opt. l. i. p. 2. Prop. 3. SMITH. r. 399. WOOD, 105.)

Corollary. Hence, when the refractions out of two mediums into a third are given, the refraction at the common surface of these mediums may be thus found. Let the refractions given

be as $m : n$, and as $m' : n'$; then the ratio sought will be that of $m n' : m' n$. For instance, let the three mediums be glass, water, and air; then $m = 3$, $n = 2$, $m' = 4$, $n' = 3$, $m n' = 9$, and $m' n = 8$. If the ratios be $4 : 3$, and $13 : 14$, we have $m n' : m' n :: 39 : 56$; and, dividing by $56 - 39$, we obtain $2 . 3$ and $3 . 3$ for m and $m + 1$, in Schol. 1, Prop. I.

Proposition III. *Problem.* (Plate II. Fig. 1.)

At the vertex of a given triangle (CBA), to place a given refracting surface (B), so that the incident and refracted rays may coincide with the sides of the triangle (AB and BC.)

Let the sides be called d and e; then in the base take, next to d (or AB), a portion (AE) equal to $\frac{n d}{n d + m e}$, or (AD $=$) $\frac{m d}{m d + n e}$; draw a line (EB, or DB) to the vertex, and the surface must be perpendicular to this line, whenever the problem is physically possible. When e becomes infinite, and parallel to the base, take $\frac{n d}{m}$ or $\frac{m d}{n}$ next to d, for the intersection of the radius of curvature.

Proposition IV. *Theorem.* (Fig. 2.)

In oblique refractions at spherical surfaces, the line (AI, KL,) joining the conjugate foci (A, I; K, L;) passes through the point (G), where a perpendicular from the centre (H) falls on the line (EF), bisecting the chords (BC, BD,) cut off from the incident and refracted rays.

Corollary 1. Let t and u be the cosines of incidence and refraction, the radius being 1, and d and e the respective distances of the foci of incident and refracted rays; then $e = \frac{m d u u}{m d u - n d t - n t t}$.

Corollary 2. For a plane surface, $e = \frac{m d u u}{- n t t}$.

Corollary 3. For parallel rays, $d = \infty$, and $e = \frac{m u u}{m u - n t}$.

Scholium 1. It may be observed, that the caustic by refraction stops short at its cusp, not geometrically, but physically, the total reflection interfering.

Corollary 4. Call $\frac{m u u}{m u - n t}$, b, and $\frac{n t t}{m u - n t}$, c; then $e = \frac{b d}{d - c}$, and $e - b = \frac{b c}{d - c}$; or, in words, the rectangle contained by the focal lengths of parallel rays, passing and repassing any surface in the same lines, is equal to the rectangle contained by the differences between these lengths and the distances of any conjugate foci.

Corollary 5. For perpendicular rays, $e = \frac{m d}{d - n} = m + \frac{m n}{d - n}$; or, if the radius be a, $e = \frac{m a d}{d - n a}$; and if d and e be given to find the radius, $a = \frac{d e}{m d + n e}$.

Corollary 6. For rays perpendicular and parallel, $e = m$, or $e = m a$.

Corollary 7. For a double convex lens, neglecting the thickness, call the first radius g, the second b, and $e = \frac{n d g b}{d g + d b - n g b}$. Hence $n = \frac{d e}{d + e} \cdot \frac{g + b}{g b}$; and, for parallel rays, $e = \frac{n g b}{g + b}$, and $n = e \cdot \frac{g + b}{g b}$. If $g = b = a$, $e = \frac{n a d}{2 d - n a}$; and for parallel rays $e = \frac{n a}{2}$: calling this principal focal length b, $e = \frac{b d}{d - b}$, as in Cor. 4; whence we have the joint focus of two lenses; also, $b = \frac{d e}{d + e}$.

Corollary 8. In a sphere, $e = m a \cdot \frac{d + a}{2 d - (m - 2) a}$, for the distance from the centre, and $b = \frac{m a}{2}$.

Scholium 2. In all these cases, if the rays converge, d must be negative. For instance, to find the joint focus of two convex, or concave lenses, the expression becomes, $e = \frac{bd}{b+d}$.

Corollary 9. In Cor. 3, the divisor becomes ultimately constant; and, when the inclination is small, the focus varies as uu.

Corollary 10. For parallel rays falling obliquely on a double convex, or double concave lens, of inconsiderable thickness, the radius being 1, $e = \frac{ntu}{2(mu - nt)}$; which varies ultimately as the product of the cosines, or as $\frac{m+n}{nn}t + t^2$.

Scholium 3. In the double convex lens, the thickness diminishes the effect of the obliquity near the axis; in the double concave, it increases it.

Scholium 4. No spherical surface, excepting one particular case, (WOOD, 155,) can collect an oblique pencil of rays, even to a physical point. The oblique rays which we have hitherto considered, are only such as lie in that section of the pencil which is made by a plane passing through the centre and the radiant point. They continue in this plane, notwithstanding the refraction, and therefore will not meet the rays of the collateral sections, till they arrive at the axis. The remark was made by Sir ISAAC NEWTON, and extended by Dr. SMITH, (SMITH r. 493, 494;) it appears, however, to have been too little noticed. (WOOD, 362.) The geometrical focus thus becomes a line, a circle, an oval, or other figure, according to the form of the pencil, the nature of the surface, and the place of the plane receiving the image. Some of the varieties of the focal image of a cylindrical pencil obliquely refracted are shown in Plate VI. Fig. 28.

Corollary 11. Hence the line joining the remoter conjugate foci, will always pass through the centre. The distance of the remoter focus of parallel rays will be expressed by $f = \frac{m}{mu - nt}$; and the least circle of aberration will be at the distance $\frac{1 + u^2 - 2u^4}{(1 + uu) \cdot (mu - nt)}$, dividing the length of aberration in the ratio of the distance of its limits from the surface. In the case of Cor. 10. $f = \frac{n}{2(mu - nt)}$.

Corollary 12. This proposition extends also to reflected rays; and, in that case, the line from the centre passes through the point of incidence.

Proposition V. *Problem.*

To find the place and magnitude of the image of a small object, after refraction at any number of spherical surfaces.

Construction. (Plate II. Fig. 3.) From any point (B) in the object (AB), draw lines to (C), the centre of the first surface, and to (D), the focus of parallel rays coming in a contrary direction: from the intersection of the second line (BD) with the tangent (EF) at the vertex, draw a line (EH) parallel to the axis, and it will cut the first line (BC) in (H), the first image of the point (B). Proceed with this image as a new object, and repeat the operation for each surface, and the last point will be in the image required. For calculation, find the place of the image by Cor. 5. Prop. IV. and its magnitude will be to that of the object, as their respective distances from the centre.

Corollary. If a confused image be received on any given plane, its magnitude will be determined by the line drawn from the preceding image through the centre of the last surface.

Proposition VI. *Problem.*

To determine the law by which the refraction at a spherical surface must vary, so as to collect parallel rays to a perfect focus.

Solution. Let v be the versed sine to the radius 1; then, at each point without the axis, n remaining the same, m must become $\sqrt{mm \pm 2nv}$; and all the rays will be collected in the principal focus.

Corollary. The same law will serve for a double convex lens, in the case of equidistant conjugate foci, substituting n for m.

Proposition VII. *Problem.*

To find the principal focus of a sphere, or lens, of which the internal parts are more dense than the external.

Solution. In order that the focal distance may be finite, the density of a finite portion about the centre must be equable: call the radius of this portion $\frac{1}{l}$, that of the sphere being unity; let the whole refraction out of the surrounding medium into this central part, be as m to n; take $r = \frac{\log. l}{\log m - \log. n}$, and let the density be supposed to vary every where inversely as the power $\frac{1}{r}$ of the distance from the centre: then the principal focal distance from the centre will be $\frac{r-1}{2} \cdot \frac{m}{nl - m}$. When $r = 1$, it becomes $\frac{1}{2(\text{H. L.}\, m - \text{H. L.}\, n)}$. For a lens, deduct one fourth of the difference between its axis and the diameter of the sphere of which its surfaces are portions.

Corollary. If the density be supposed to vary suddenly at the surface, m must express the difference of the refractions at the

centre and at the surface; and the focal distance, thus determined, must be diminished according to the refraction at the surface.

Proposition VIII. *Problem.*

To find the nearer focus of parallel rays falling obliquely on a sphere of variable density.

Solution. Let r be as in the last proposition, s the sine of incidence, t the cosine, and e the distance of the focus from the point of emersion. Then $e = \frac{w - t}{2 - tw}$, w being $= \frac{2}{(r-1)s^{\frac{r+1}{r-1}}}$.
$(aA + bB + cC + \ldots) + 2aA + 6bBs^2 + 10cCs^4 + \ldots$, where $a = \frac{r}{r+1}$, $b = \frac{r}{3r-1}$, $c = \frac{r}{5r-3}$, $A = 1$, $B = \frac{1}{2}A$, $C = \frac{3}{4}B$, $D = \frac{5}{6}C$. But, when s is large, the latter part of the series converges somewhat slowly. The former part might be abridged if it were necessary: but, since the focus in this case is always very imperfect, it is of the less consequence to provide an easy calculation.

General Scholium. The two first propositions relate to well known phenomena; the third can hardly be new; the fourth approaches the nearest to MACLAURIN's construction, but is far more simple and convenient; the fifth and sixth have no difficulty; but the two last require a long demonstration. The one is abridged by a property of logarithms; the other is derived from the laws of centripetal forces, on the supposition of velocities directly as the refractive densities, correcting the series for the place of the apsis, and making the sine of incidence variable, to determine the fluxion of the angle of deviation.

V. Dr. PORTERFIELD has employed an experiment, first made by SCHEINER, to the determination of the focal distance

of the eye; and has described, under the name of an optometer, a very excellent instrument, founded on the principle of the phenomenon.* But the apparatus is capable of considerable improvement; and I shall beg leave to describe an optometer, simple in its construction, and equally convenient and accurate in its application.

Let an obstacle be interposed between a radiant point (R, Plate II. Fig. 4,) and any refracting surface, or lens (CD), and let this obstacle be perforated at two points (A and B) only. Let the refracted rays be intercepted by a plane, so as to form an image on it. Then it is evident, that when this plane (EF) passes through the focus of refracted rays, the image formed on it will be a single point. But, if the plane be advanced forwards (to GH), or removed backwards (to IK), the small pencils passing through the perforations, will no longer meet in a single point, but will fall on two distinct spots of the plane (G, H; I, K;) and, in either case, form a double image of the object.

Let us now add two more radiating points, (S and T, Fig. 5,) the one nearer to the lens than the first point, the other more remote; and, when the plane which receives the images passes through the focus of rays coming from the first point, the images of the second and third points must both be double (*s s*, *t t*;) since the plane (EF) is without the focal distance of rays coming from the furthest point, and within that of rays coming from the nearest. Upon this principle, Dr. PORTERFIELD's optometer was founded.

But, if the three points be supposed to be joined by a line, and this line to be somewhat inclined to the axis of the lens,

* Edinb. Med. Ess. Vol. IV. p. 185.

each point of the line, except the first point (R, Fig. 6,) will have a double image; and each pair of images, being contiguous to those of the neighbouring radiant points, will form with them two continued lines, and the images being more widely separated as the point which they represent is further from the first radiant point, the lines (*s t*, *s t*,) will converge on each side towards (*r*) the image of this point, and there will intersect each other.

The same happens when we look at any object through two pin holes, within the limits of the pupil. If the object be at the point of perfect vision, the image on the retina will be single; but, in every other case, the image being double, we shall appear to see a double object: and, if we look at a line pointed nearly to the eye, it will appear as two lines, crossing each other in the point of perfect vision. For this purpose, the holes may be converted into slits, which render the images nearly as distinct, at the same time that they admit more light. The number may be increased from two to four, or more, whenever particular investigations render it necessary.

The optometer may be made of a slip of card-paper, or of ivory, about eight inches in length, and one in breadth, divided longitudinally by a black line, which must not be too strong. The end of the card must be cut as is shown in Plate III. Fig 7, in order that it may be turned up, and fixed in an inclined position by means of the shoulders: or a detached piece, nearly of this form, may be applied to the optometer, as it is here engraved. A hole about half an inch square must be made in this part; and the sides so cut as to receive a slider of thick paper, with slits of different sizes, from a fortieth to a tenth of an inch in breadth, divided by spaces somewhat broader; so that each observer may choose that which best suits the aperture of his pupil.

In order to adapt the instrument to the use of presbyopic eyes, the other end must be furnished with a lens of four inches focal length; and a scale must be made near the line on each side of it, divided from one end into inches, and from the other according to the table here calculated from Cor. 7. Prop. IV, by means of which, not only diverging, but also parallel and converging rays from the lens are referred to their virtual focus. The instrument is easily applicable to the purpose of ascertaining the focal length of spectacles required for myopic or presbyopic eyes. Mr. Cary has been so good as to furnish me with the numbers and focal lengths of the glasses commonly made; and I have calculated the distances at which those numbers must be placed on the scale of the optometer, so that a presbyopic eye may be enabled to see at eight inches distance, by using the glasses of the focal length placed opposite to the nearest crossing of the lines; and a myopic eye with parallel rays, by using the glasses indicated by the number that stands opposite their furthest crossing. To facilitate the observation, I have also placed these numbers opposite that point which will be the nearest crossing to myopic eyes; but this, upon the arbitrary supposition of an equal capability of change of focus in every eye, which I must confess is often far from the truth. It cannot be expected, that every person, on the first trial, will fix precisely upon that power which best suits the defect of his sight. Few can bring their eyes at pleasure to the state of full action, or of perfect relaxation; and a power two or three degrees lower than that which is thus ascertained, will be found sufficient for ordinary purposes. I have also added to the second table, such numbers as will point out the spectacles necessary for a presbyopic eye, to see at twelve and at eighteen inches respectively: the middle series will perhaps be the most

proper for placing the numbers on the scale. The optometer should be applied to each eye; and, at the time of observing, the opposite eye should not be shut, but the instrument should be screened from its view. The place of intersection may be accurately ascertained, by means of an index sliding along the scale.

The optometer is represented in Plate III. Fig. 8 and 9; and the manner in which the lines appear, in Fig. 10.

Table I. *For extending the scale by a lens of 4 inches focus.*

4	2.00	11	2.93	30	3.52	200	3.92	−35	4.51	−12	6.00
5	2.22	12	3.00	40	3.64	∞	4.00	−30	4.62	−11	6.29
6	2.40	13	3.06	50	3.70	−200	4.08	−25	4.76	−10	6.67
7	2.55	14	3.11	60	3.75	−100	4.17	−20	5.00	−9.5	6.90
8	2.67	15	3.16	70	3.78	−50	4.35	−15	5.45	−9.0	7.20
9	2.77	20	3.33	80	3.81	−45	4.39	−14	5.60	−8.5	7.56
10	2.86	25	3.45	100	3.85	−40	4.44	−13	5.78	−8.0	8.00

Table II. *For placing the numbers indicating the focal length of convex glasses.*

Foc.	VIII.	XII	XVIII.	Foc.	VIII.	XII.	XVIII.	Foc.	VIII.	XII.	XVIII.
0	8.00	12.00	18.00	20	13.33	30.00	180.00	8	∞	−24.00	−14.40
40	10.00	17.14	22.73	18	14.40	36.00	∞	7	−56.00	−16.80	−11.45
36	10.28	18.00	36.00	16	16.00	48.00	−144.00	6	−24.00	−12.00	− 9.00
30	10.91	20.00	45.00	14	18.67	84.00	− 63.00	5	−13.33	− 8.57	− 5.92
28	11.20	21.00	50.40	12	24.00	∞	− 36.00	4.5	−10.29	− 7.20	− 6.00
26	11.56	22.29	58.50	11	29.33	−132.00	− 28.29	4.0	− 8.00	− 6.00	− 5.14
24	12.00	24.00	72.00	10	40.00	− 60.00	− 22.50	3.5	− 6.22	− 4.94	− 4.34
22	12.77	26.40	99.00	9	72.00	− 36.00	− 18.00	3.0	− 4.80	− 4.00	− 3.6

Table III. *For concave glasses.*

Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.	Number.	Focus and furthest place.	Nearest place.
0		4.00	7	8	2.67	14	3.00	1.71
1	24	3.43	8	7	2.54	15	2.75	1.63
2	18	3.27	9	6	2.40	16	2.50	1.54
3	16	3.20	10	5	2.22	17	2.25	1.44
4	12	3.00	11	4.5	2.12	18	2.00	1.33
5	10	2.86	12	4.0	2.00	19	1.75	1.22
6	9	2.77	13	3.5	1.87	20	1.50	1.02

VI. Being convinced of the advantage of making every observation with as little assistance as possible, I have endeavoured to confine most of my experiments to my own eyes; and I shall, in general, ground my calculations on the supposition of an eye nearly similar to my own. I shall therefore first endeavour to ascertain all its dimensions, and all its faculties.

For measuring the diameters, I fix a small key on each point of a pair of compasses; and I can venture to bring the rings into immediate contact with the sclerotica. The transverse diameter is externally 98 hundredths of an inch.

To find the axis, I turn the eye as much inwards as possible, and press one of the keys close to the sclerotica, at the external angle, till it arrives at the spot where the spectrum formed by its pressure coincides with the direction of the visual axis, and, looking in a glass, I bring the other key to the cornea. The optical axis of the eye, making allowance of three hundredths for the coats, is thus found to be 91 hundredths of an inch, from the external surface of the cornea to the retina. With an eye less prominent, this method might not have succeeded.

The vertical diameter, or rather chord, of the cornea, is 45 hundredths: its versed sine 11 hundredths. To ascertain the versed sine, I looked with the right eye at the image of the left, in a small speculum held close to the nose, while the left eye was so averted that the margin of the cornea appeared as a straight line, and compared the projection of the cornea with the image of a cancellated scale held in a proper direction behind the left eye, and close to the left temple. The horizontal chord of the cornea is nearly 49 hundredths.

Hence the radius of the cornea is 31 hundredths. It may

be thought that I assign too great a convexity to the cornea; but I have corrected it by a number of concurrent observations, which will be enumerated hereafter.

The eye being directed towards its image, the projection of the margin of the sclerotica is 22 hundredths from the margin of the cornea, towards the external angle, and 27 towards the internal angle of the eye: so that the cornea has an eccentricity of one fortieth of an inch, with respect to the section of the eye perpendicular to the visual axis.

The aperture of the pupil varies from 27 to 13 hundredths; at least this is its apparent size, which must be somewhat diminished, on account of the magnifying power of the cornea, perhaps to 25 and 12. When dilated, it is nearly as eccentric as the cornea; but, when most contracted, its centre coincides with the reflection of an image from an object held immediately before the eye; and this image very nearly with the centre of the whole apparent margin of the sclerotica: so that the cornea is perpendicularly intersected by the visual axis.

My eye, in a state of relaxation, collects to a focus on the retina, those rays which diverge vertically from an object at the distance of ten inches from the cornea, and the rays which diverge horizontally from an object at seven inches distance. For, if I hold the plane of the optometer vertically, the images of the line appear to cross at ten inches; if horizontally, at seven. The difference is expressed by a focal length of 23 inches. I have never experienced any inconvenience from this imperfection, nor did I ever discover it till I made these experiments; and I believe I can examine minute objects with as much accuracy as most of those whose eyes are differently formed. On mentioning it to Mr. CARY, he informed me, that he had

frequently taken notice of a similar circumstance; that many persons were obliged to hold a concave glass obliquely, in order to see with distinctness, counterbalancing, by the inclination of the glass, the too great refractive power of the eye in the direction of that inclination, (Cor. 10. Prop. IV.) and finding but little assistance from spectacles of the same focal length. The difference is not in the cornea, for it exists when the effect of the cornea is removed by a method to be described hereafter. The cause is, without doubt, the obliquity of the uvea, and of the crystalline lens, which is nearly parallel to it, with respect to the visual axis: this obliquity will appear, from the dimensions already given, to be about 10 degrees. Without entering into a very accurate calculation, the difference observed is found (by the same corollary) to require an inclination of about 13 degrees; and the remaining three degrees may easily be added, by the greater obliquity of the posterior surface of the crystalline opposite the pupil. There would be no difficulty in fixing the glasses of spectacles, or the concave eye-glass of a telescope, in such a position as to remedy the defect.

In order to ascertain the focal distance of the lens, we must assign its probable distance from the cornea. Now the versed sine of the cornea being 11 hundredths, and the uvea being nearly flat, the anterior surface of the lens must probably be somewhat behind the chord of the cornea; but by a very inconsiderable distance, for the uvea has the substance of a thin membrane, and the lens approaches very near to it: we will therefore call this distance 12 hundredths. The axis and proportions of the lens must be estimated by comparison with anatomical observations; since they affect, in a small degree, the determination of its focal distance. M. PETIT found the axis

almost always about two lines, or 18 hundredths of an inch. The radius of the anterior surface was in the greatest number 3 lines, but oftener more than less. We will suppose mine to be $3\frac{1}{4}$, or nearly $\frac{3}{10}$ of an inch. The radius of the posterior surface was most frequently $2\frac{1}{2}$ lines, or $\frac{2}{9}$ of an inch.* The optical centre will be therefore $\left(\frac{18 \times 30}{30 + 22} =\right)$ about one-tenth of an inch from the anterior surface: hence we have 22 hundredths, for the distance of the centre from the cornea. Now, taking 10 inches as the distance of the radiant point, the focus of the cornea will be 115 hundredths behind the centre of the lens. (Cor. 5. Prop. IV.) But the actual joint focus is $(91 - 22 =)$ 69 behind the centre: hence, disregarding the thickness of the lens, its principal focal distance is 173 hundredths. (Cor. 7. Prop. IV.) For its refractive power in the eye, we have (by Cor. 7. Prop. IV.) $n = 13{,}5$, and $m = 14{,}5$. Calculating upon this refractive power, with the consideration of the thickness also, we find that it requires a correction, and comes near to the ratio of 14 to 13 for the sines. It is well known that the refractive powers of the humours are equal to that of water; and, that the thickness of the cornea is too equable to produce any effect on the focal distance.

For determining the refractive power of the crystalline lens by a direct experiment, I made use of a method suggested to me by Dr. Wollaston. I found the refractive power of the centre of the recent human crystalline to that of water, as 21 to 20. The difference of this ratio from the ratio of 14 to 13, ascertained from calculation, is probably owing to two circumstances. The first is, that the substance of the lens being in some degree soluble in water, a portion of the aqueous fluid

* Mem. de l'Acad. de Paris, 1730. p. 6. Ed. Amst.

within its capsule penetrates after death, so as somewhat to lessen the density. When dry, the refractive power is little inferior to that of crown glass. The second circumstance is, the unequal density of the lens. The ratio of 14 to 13 is founded on the supposition of an equable density: but, the central part being the most dense, the whole acts as a lens of smaller dimensions; and it may be found by Prop. VII. that if the central portion of a sphere be supposed of uniform density, refracting as 21 to 20, to the distance of one half of the radius, and the density of the external parts to decrease gradually, and at the surface to become equal to that of the surrounding medium, the sphere thus constituted, will be equal in focal length to a uniform sphere of the same size, with a refraction of 16 to 15 nearly. And the effect will be nearly the same, if the central portion be supposed to be smaller than this, but the density to be somewhat greater at the surface than that of the surrounding medium, or to vary more rapidly externally than internally. On the whole, it is probable that the refractive power of the centre of the human crystalline, in its living state, is to that of water nearly as 18 to 17; that the water imbibed after death, reduces it to the ratio of 21 to 20; but that, on account of the unequable density of the lens, its effect in the eye is equivalent to a refraction of 14 to 13 for its whole size. Dr. WOLLASTON has ascertained the refraction out of air, into the centre of the recent crystalline of oxen and sheep, to be nearly as 143 to 100; into the centre of the crystalline of fish, and into the dried crystalline of sheep, as 152 to 100. Hence, the refraction of the crystalline of oxen in water, should be as 15 to 14: but the human crystalline, when recent, is decidedly less refractive.

These considerations will explain the inconsistency of different observations on the refractive power of the crystalline: and, in particular, how the refraction which I formerly calculated, from measuring the focal length of the lens,* is so much greater than that which is determined by other means. But, for direct experiments, Dr. WOLLASTON's method is exceedingly accurate.

When I look at a minute lucid point, such as the image of a candle in a small concave speculum, it appears as a radiated star, as a cross, or as an unequal line, and never as a perfect point, unless I apply a concave lens inclined at a proper angle, to correct the unequal refraction of my eye. If I bring the point very near, it spreads into a surface nearly circular, and almost equably illuminated, except some faint lines, nearly in a radiating direction. For this purpose, the best image is a candle, or a small speculum, viewed through a minute lens at some little distance, or seen by reflection in a larger lens. If any pressure has been applied to the eye, such as that of the finger keeping it shut, the sight is often confused for a short time after the removal of the finger, and the image is in this case spotty or curdled. The radiating lines are probably occasioned by some slight inequalities in the surface of the lens, which is very superficially furrowed in the direction of its fibres: the curdled appearance will be explained hereafter. When the point is further removed, the image becomes evidently oval, the vertical diameter being longest, and the lines a little more distinct than before, the light being strongest in the neighbourhood of the centre; but immediately at the centre there is a darker spot, owing to such a slight depression at the vertex as is often

* Phil. Trans. for 1793. p. 174.

observable in examining the lens after death. The situation of the rays is constant, though not regular; the most conspicuous are seven or eight in number; sometimes about twenty fainter ones may be counted. Removing the point a little further, the image becomes a short vertical line; the rays that diverged horizontally being perfectly collected, while the vertical rays are still separate. In the next stage, which is the most perfect focus, the line spreads in the middle, and approaches nearly to a square, with projecting angles, but is marked with some darker lines towards the diagonals. The square then flattens into a rhombus, and the rhombus into a horizontal line unequally bright. At every greater distance, the line lengthens, and acquires also breadth, by radiations shooting out from it, but does not become a uniform surface, the central part remaining always considerably brightest, in consequence of the same flattening of the vertex which before made it fainter. Some of these figures bear a considerable analogy to the images derived from the refraction of oblique rays, (Schol. 4. Prop. IV.) and still more strongly resemble a combination of two of them in opposite directions; so as to leave no doubt, but that both surfaces of the lens are oblique to the visual axis, and co-operate in distorting the focal point. This may also be verified, by observing the image delineated by a common glass lens, when inclined to the incident rays. (See Plate VI. Fig. 28—40.)

The visual axis being fixed in any direction, I can at the same time see a luminous object placed laterally at a considerable distance from it; but in various directions the angle is very different. Upwards it extends to 50 degrees, inwards to 60, downwards to 70, and outwards to 90 degrees. These internal limits of the field of view nearly correspond with

the external limits formed by the different parts of the face, when the eye is directed forwards and somewhat downwards, which is its most natural position; although the internal limits are a little more extensive than the external; and both are well calculated for enabling us to perceive the most readily, such objects as are the most likely to concern us. Dr. WOLLASTON's eye has a larger field of view, both vertically and horizontally, but nearly in the same proportions, except that it extends further upwards. It is well known, that the retina advances further forwards towards the internal angle of the eye, than towards the external angle; but upwards and downwards its extent is nearly equal, and is indeed every way greater than the limits of the field of view, even if allowance is made for the refraction of the cornea only. The sensible portion seems to coincide more nearly with the painted choroid of quadrupeds: but the whole extent of perfect vision is little more than 10 degrees; or, more strictly speaking, the imperfection begins within a degree or two of the visual axis, and at the distance of 5 or 6 degrees becomes nearly stationary, until, at a still greater distance, vision is wholly extinguished. The imperfection is partly owing to the unavoidable aberration of oblique rays, but principally to the insensibility of the retina: for, if the image of the sun itself be received on a part of the retina remote from the axis, the impression will not be sufficiently strong to form a permanent spectrum, although an object of very moderate brightness will produce this effect when directly viewed. It would probably have been inconsistent with the economy of nature, to bestow a larger share of sensibility on the retina. The optic nerve is at present very large; and the delicacy of the organ renders it, even at present, very susceptible of injury from slight irritation,

and very liable to inflammatory affections; and, in order to make the sight so perfect as it is, it was necessary to confine that perfection within narrow limits. The motion of the eye has a range of about 55 degrees in every direction; so that the field of perfect vision, in succession, is by this motion extended to 110 degrees.

But the whole of the retina is of such a form as to receive the most perfect image, on every part of its surface, that the state of each refracted pencil will admit; and the varying density of the crystalline renders that state more capable of delineating such a picture, than any other imaginable contrivance could have done. To illustrate this, I have constructed a diagram, representing the successive images of a distant object filling the whole extent of view, as they would be formed by the successive refractions of the different surfaces. Taking the scale of my own eye, I am obliged to substitute, for a series of objects at any indefinitely great distance, a circle of 10 inches radius; and it is most convenient to consider only those rays which pass through the anterior vertex of the lens; since the actual centre of each pencil must be in the ray which passes through the centre of the pupil, and the short distance of the vertex of the lens from this point, will always tend to correct the unequal refraction of oblique rays. The first curve (Plate IV. Fig. 16.) is the image formed by the furthest intersection of rays refracted at the cornea; the second, the image formed by the nearest intersection; the distance between these, shows the degree of confusion in the image; and the third curve, its brightest part. Such must be the form of the image which the cornea tends to delineate in an eye deprived of the crystalline lens; nor can any external remedy properly correct the imperfection of lateral

vision. The next three curves show the images formed after the refraction at the anterior surface of the lens, distinguished in the same manner; and the three following, the result of all the successive refractions. The tenth curve is a repetition of the ninth, with a slight correction near the axis, at F, where, from the breadth of the pupil, some perpendicular rays must fall. By comparing this with the eleventh, which is the form of the retina, it will appear that nothing more is wanting for their perfect coincidence, than a moderate diminution of density in the lateral parts of the lens. If the law, by which this density varies, were more accurately ascertained, its effect on the image might be calculated from the eighth proposition; but the operations would be somewhat laborious: probably the image, thus corrected, would approach very nearly to the form of the twelfth curve.

To find the place of the entrance of the optic nerve, I fix two candles at ten inches distance, retire sixteen feet, and direct my eye to a point four feet to the right or left of the middle of the space between them: they are then lost in a confused spot of light; but any inclination of the eye brings one or the other of them into the field of view. In BERNOULLI's eye, a greater deviation was required for the direction of the axis;* and the obscured part appeared to be of greater extent. From the experiment here related, the distance of the centre of the optic nerve from the visual axis is found (by Prop. V.) to be 16 hundredths of an inch; and the diameter of the most insensible part of the retina, one-thirtieth of an inch. In order to ascertain the distance of the optic nerve from the point opposite to the pupil, I took the sclerotica of the human eye, divided it into segments, from the centre of the cornea towards the optic nerve, and extended it on a plane. I then measured the longest and shortest

* Comm. Petrop. I. p. 314.

distances from the cornea to the perforation made by the nerve, and their difference was exactly one-fifth of an inch. To this we must add a fiftieth, on account of the eccentricity of the pupil in the uvea, which in the eye that I measured was not great, and the distance of the centre of the nerve from the point opposite the pupil will be 11 hundredths. Hence it appears, that the visual axis is five hundredths, or one-twentieth of an inch, further from the optic nerve than the point opposite the pupil. It is possible that this distance may be different in different eyes: in mine, the obliquity of the lens, and the eccentricity of the pupil with respect to it, will tend to throw a direct ray upon it, without much inclination of the whole eye; and it is not improbable, that the eye is also turned slightly outwards, if looking at any object before it, although the inclination is too small to be subjected to measurement.

It must also be observed, that it is very difficult to ascertain the proportions of the eye so exactly as to determine, with certainty, the size of an image on the retina; the situation, curvature, and constitution of the lens, make so material a difference in the result, that there may possibly be an error of almost one-tenth of the whole. In order, therefore, to obtain some confirmation from experiment, I placed two candles at a small distance from each other, turned the eye inwards, and applied the ring of a key so as to produce a spectrum, of which the edge coincided with the inner candle; then, fixing my eye on the outward one, I found that the spectrum advanced over two-sevenths of the distance between them. Hence, the same portion of the retina that subtended an angle of seven parts at the centre of motion of the eye, subtended an angle of five at the supposed intersection of the principal rays; (Plate III. Fig. 11.) and the

distance of this intersection from the retina was 637 thousandths. This nearly corresponds with the former calculation; nor can the distance of the centre of the optic nerve from the point of most perfect vision be, on any supposition, much less than that which is here assigned. And, in the eyes of quadrupeds, the most strongly painted part of the choroid is further from the nerve than the real axis of the eye.

I have endeavoured to express in four figures, the form of every part of my eye, as nearly as I have been able to ascertain it; the first (Pl. V. Fig. 17.) is a vertical section; the second (Fig. 18.) a horizontal section; the third and fourth are front views, in different states of the pupil. (Fig. 19 and 20.)

Considering how little inconvenience is experienced from so material an inequality in the refraction of the lens as I have described, we have no reason to expect a very accurate provision for correcting the aberration of the lateral rays. But, as far as can be ascertained by the optometer, the aberration arising from figure is completely corrected; since four or more images of the same line appear to meet exactly in the same point, which they would not do if the lateral rays were materially more refracted than the rays near the axis. The figure of the surfaces is sometimes, and perhaps always, more or less hyperbolical* or elliptical: in the interior laminæ indeed, the solid angle of the margin is somewhat rounded off; but the weaker refractive power of the external parts, must greatly tend to correct the aberration arising from the too great curvature towards the margin of the disc. Had the refractive power been uniform, it might have collected the lateral rays of a direct pencil nearly as well; but it would have been less adapted to oblique pencils of

* Petit Mém. de l'Acad. 1725, p. 20.

rays; and the eye must also have been encumbered with a mass of much greater density than is now required, even for the central parts: and, if the whole lens had been smaller, it would also have admitted too little light. It is possible too, that Mr. Ramsden's observation,* on the advantage of having no reflecting surface, may be well-founded: but it has not been demonstrated, that less light is lost in passing through a medium of variable density, than in a sudden transition from one part of that medium to another; nor are we yet sufficiently acquainted with the cause of this reflection, to be enabled to reason satisfactorily on the subject. But, neither this gradation, nor any other provision, has the effect of rendering the eye perfectly achromatic. Dr. Jurin had remarked this, long ago,† from observing the colour bordering the image of an object seen indistinctly. Dr. Wollaston pointed out to me on the optometer, the red and blue appearance of the opposite internal angles of the crossing lines; and mentioned, at the same time, a very elegant experiment for proving the dispersive power of the eye. He looks through a prism at a small lucid point, which of course becomes a linear spectrum. But the eye cannot so adapt itself as to make the whole spectrum appear a line; for, if the focus be adapted to collect the red rays to a point, the blue will be too much refracted, and expand into a surface; and the reverse will happen if the eye be adapted to the blue rays; so that, in either case, the line will be seen as a triangular space. The observation is confirmed, by placing a small concave speculum in different parts of a prismatic spectrum, and ascertaining the utmost distances at which the eye can collect the rays of different colours to a focus. By these means I find, that the red rays, from a point at

* Phil. Trans. for 1795, p. 2.

† Smith, c. 96.

12 inches distance, are as much refracted as white or yellow light at 11. The difference is equal to the refraction of a lens 132 inches in focus. But the aberration of the red rays in a lens of crown glass, of equal mean refractive power with the eye, would be equivalent to the effect of a lens 44 inches in focus. If, therefore, we can depend upon this calculation, the dispersive power of the eye collectively, is one-third of the dispersive power of crown glass, at an equal angle of deviation. I cannot observe much aberration in the violet rays. This may be, in part, owing to their faintness; but yet I think their aberration must be less than that of the red rays. I believe it was Mr. RAMSDEN's opinion, that since the separation of coloured rays is only observed where there is a sudden change of density, such a body as the lens, of a density gradually varying, would have no effect whatever in separating the rays of different colours. If this hypothesis should appear to be well-founded, we must attribute the whole dispersion to the aqueous humour; and its dispersive power will be half that of crown glass, at the same deviation. But we have an instance, in the atmosphere, of a very gradual change of density; and yet Mr. GILPIN informs me, that the stars, when near the horizon, appear very evidently coloured. At a more favourable season of the year, it would not be difficult to ascertain, by means of the optometer, the dispersive power of the eye, and of its different parts, with greater accuracy than by the experiment here related. Had the dispersive power of the whole eye been equal to that of flint glass, the distances of perfect vision would have varied from 12 inches to 7 for different rays, in the same state of the mean refractive powers.

VII. The faculty of accommodating the eye to various

distances, appears to exist in very different degrees in different individuals. The shortest distance of perfect vision in my eye, is 26 tenths of an inch for horizontal, and 29 for vertical rays. This power is equivalent to the addition of a lens of 4 inches focus. Dr. WOLLASTON can see at 7 inches, and with converging rays; the difference answering to 6 inches focal length. Mr. ABERNETHY has perfect vision from 3 inches to 30, or a power equal to that of a lens $3\frac{1}{3}$ inches in focus. A young lady of my acquaintance can see at 2 inches and at 4; the difference being equivalent to 4 inches focus. A middle aged lady at 3 and at 4; the power of accommodation being only equal to the effect of a lens of 12 inches focus. In general, I have reason to think, that the faculty diminishes in some degree, as persons advance in life; but some also of a middle age appear to possess it in a very small degree. I shall take the range of my own eye, as being probably about the medium, and inquire what changes will be necessary in order to produce it; whether we suppose the radius of the cornea to be diminished, or the distance of the lens from the retina to be increased, or these two causes to act conjointly, or the figure of the lens itself to undergo an alteration.

1. We have calculated, that when the eye is in a state of relaxation, the refraction of the cornea is such as to collect rays diverging from a point ten inches distant, to a focus at the distance of $13\frac{2}{3}$ tenths. In order that it may bring to the same focus, rays diverging from a point distant 29 tenths, we find (by Cor. 5, Prop. IV.) that its radius must be diminished from 31 to 25 hundredths, or very nearly in the ratio of five to four.

2. Supposing the change from perfect vision at ten inches to 29 tenths, to be effected by a removal of the retina to a greater

distance from the lens, this will require, (by the same Corollary,) an elongation of 135 thousandths, or more than one-seventh of the diameter of the eye. In Mr. Abernethy's eye, an elongation of 17 hundredths, or more than one-sixth, is requisite.

3. If the radius of the cornea be diminished one-sixteenth, or to 29 hundredths, the eye must at the same time be elongated 97 thousandths, or about one-ninth of its diameter.

4. Supposing the crystalline lens to change its form; if it became a sphere, its diameter would be 28 hundredths, and, its anterior surface retaining its situation, the eye would have perfect vision at the distance of an inch and a half. (Cor. 5 and 8, Prop. IV.) This is more than double the actual change. But it is impossible to determine precisely how great an alteration of form is necessary, without ascertaining the nature of the curves into which its surfaces may be changed. If it were always a spheroid more or less oblate, the focal length of each surface would vary inversely as the square of the axis: but, if the surfaces became, from spherical, portions of hyperbolic conoids, or of oblong spheroids, or changed from more obtuse to more acute figures of this kind, the focal length would vary more rapidly. Disregarding the elongation of the axis, and supposing the curvature of each surface to be changed proportionally, the radius of the anterior must become about 24, and that of the posterior 17 hundredths.

VIII. I shall now proceed to inquire, which of these changes takes place in nature; and I shall begin with a relation of experiments made in order to ascertain the curvature of the cornea in all circumstances.

The method described in Mr. Home's Croonian Lecture for

1795,* appears to be far preferable to the apparatus of the preceding year: † for a difference in the distance of two images seen in the cornea, would be far greater, and more conspicuous, than a change of its prominency, and far less liable to be disturbed by accidental causes. It is nearly, and perhaps totally impossible to change the focus of the eye, without some motion of its axis. The eyes sympathize perfectly with each other; and the change of focus is almost inseparable from a change of the relative situation of the optic axes; so much, that if I direct both my eyes at an object beyond their furthest focus, I cannot avoid bringing that focus a little nearer: while one axis moves, it is not easy to keep the other perfectly at rest; and it is not impossible, that a change in the proportions of some eyes, may render a slight alteration of the position of the axis absolutely necessary. These considerations may partly explain the trifling difference in the place of the cornea that was observed in 1794. It appears that the experiments of 1795 were made with considerable accuracy, and no doubt with excellent instruments; and their failing to ascertain the existence of any change, induced Mr. HOME and Mr. RAMSDEN to abandon, in great measure, the opinion which suggested them, and to suppose, that a change of the cornea produces only one-third of the effect. Dr. OLBERS of Bremen, who in the year 1780 published a most elaborate dissertation on the internal changes of the eye, ‡ which he lately presented to the Royal Society, had been equally unsuccessful in his attempts to measure this change of the cornea, at the same time that his opinion was in favour of its existence.

* Phil. Trans. for 1796, p. 2. † Phil. Trans. for 1795, p. 13.

‡ De Oculi Mutationibus internis. Gotting. 1780. 4°.

Room was however still left for a repetition of the experiments; and I began with an apparatus nearly resembling that which Mr. Home has described. I had an excellent achromatic microscope, made by Mr. Ramsden for my friend Mr. John Ellis, of five inches focal length, magnifying about 20 times. To this I adapted a cancellated micrometer, in the focus of the eye not employed in looking through the microscope: it was a large card, divided by horizontal and vertical lines into fortieths of an inch. When the image in the microscope was compared with this scale, care was taken to place the head so that the relative motion of the images on the micrometer, caused by the unsteadiness of the optic axis, should always be in the direction of the horizontal lines, and that there could be no error, from this motion, in the dimensions of the image taken vertically. I placed two candles so as to exhibit images in a vertical position in the eye of Mr. König, who had the goodness to assist me; and, having brought them into the field of the microscope, where they occupied 35 of the small divisions, I desired him to fix his eye on objects at different distances in the same direction: but I could not perceive the least variation in the distance of the images.

Finding a considerable difficulty in a proper adjustment of the microscope, and being able to depend on my naked eye in measuring distances, without an error of one 500th of an inch, I determined to make a similar experiment without any magnifying power. I constructed a divided eye-glass of two portions of a lens, so small, that they passed between two images reflected from my own eye; and, looking in a glass, I brought the apparent places of the images to coincide, and then made the change requisite for viewing nearer objects: but the images still

coincided. Neither could I observe any change in the images reflected from the other eye, where they could be viewed with greater convenience, as they did not interfere with the eye-glass. But, not being at that time aware of the perfect sympathy of the eyes, I thought it most certain to confine my observation to the one with which I saw. I must remark that, by a little habit, I have acquired a very ready command over the accommodation of my eye, so as to be able to view an object with attention, without adjusting my eye to its distance.

I also stretched two threads, a little inclined to each other, across a ring, and divided them by spots of ink into equal spaces. I then fixed the ring, applied my eye close behind it, and placed two candles in proper situations before me, and a third on one side, to illuminate the threads. Then, setting a small looking-glass, first at four inches distance, and next at two, I looked at the images reflected in it, and observed at what part of the threads they exactly reached across in each case; and with the same result as before.

I next fixed the cancellated micrometer at a proper distance, illuminated it strongly, and viewed it through a pin-hole, by which means it became distinct in every state of the eye; and, looking with the other eye into a small glass, I compared the image with the micrometer, in the manner already described. I then changed the focal distance of the eye, so that the lucid points appeared to spread into surfaces, from being too remote for perfect vision; and I noted on the scale, the distance of their centres; but that distance was invariable.

Lastly, I drew a diagonal scale, with a diamond, on a looking-glass, (Plate III. Fig. 12.) and brought the images into contact with the lines of the scale. Then, since the image of the

eye occupies on the surface of a glass half its real dimensions, at whatever distance it is viewed, its true size is always double the measure thus obtained. I illuminated the glass strongly, and made a perforation in a narrow slip of black card, which I held between the images; and was thus enabled to compare them with the scale, although their apparent distance was double that of the scale. I viewed them in all states of the eye; but I could perceive no variation in the interval between them.

The sufficiency of these methods may be thus demonstrated. Make a pressure along the edge of the upper eyelid with any small cylinder, for instance a pencil, and the optometer will show that the focus of horizontal rays is a little elongated, while that of vertical rays is shortened; an effect which can only be owing to a change of curvature in the cornea. Not only the apparatus here described, but even the eye unassisted, will be capable of discovering a considerable change in the images reflected from the cornea, although the change be much smaller than that which is requisite for the accommodation of the eye to different distances. On the whole, I cannot hesitate to conclude, that if the radius of the cornea were diminished but one-twentieth, the change would be very readily perceptible by some of the experiments related; and the whole alteration of the eye requires one-fifth.

But a much more accurate and decisive experiment remains. I take out of a small botanical microscope, a double convex lens, of eight-tenths radius and focal distance, fixed in a socket one-fifth of an inch in depth; securing its edges with wax, I drop into it a little water, nearly cold, till it is three-fourths full, and then apply it to my eye, so that the cornea enters half way into the socket, and is every where in contact with the water. (Plate III. Fig. 13.) My eye immediately becomes presbyopic, and the refractive

power of the lens, which is reduced by the water to a focal length of about 16 tenths, (Cor. 5. Prop. IV.) is not sufficient to supply the place of the cornea, rendered inefficacious by the intervention of the water; but the addition of another lens, of five inches and a half focus, restores my eye to its natural state, and somewhat more. I then apply the optometer, and I find the same inequality in the horizontal and vertical refractions as without the water; and I have, in both directions, a power of accommodation equivalent to a focal length of four inches, as before. At first sight indeed, the accommodation appears to be somewhat less, and only able to bring the eye from the state fitted for parallel rays to a focus at five inches distance; and this made me once imagine, that the cornea might have some slight effect in the natural state; but, considering that the artificial cornea was about a tenth of an inch before the place of the natural cornea, I calculated the effect of this difference, and found it exactly sufficient to account for the diminution of the range of vision. I cannot ascertain the distance of the glass lens from the cornea to the hundredth of an inch; but the error cannot be much greater, and it may be on either side.

After this, it is almost necessary to apologize for having stated the former experiments; but, in so delicate a subject, we cannot have too great a variety of concurring evidence.

IX. Having satisfied myself that the cornea is not concerned in the accommodation of the eye, my next object was to inquire if any alteration in the length of its axis could be discovered; for this appeared to be the only possible alternative: and, considering that such a change must amount to one-seventh of the diameter of the eye, I flattered myself with the expectation of submitting it to measurement. Now, if the axis of the eye

were elongated one-seventh, its transverse diameter must be diminished one-fourteenth, and the semi-diameter would be shortened a thirtieth of an inch.

I therefore placed two candles so that when the eye was turned inwards, and directed towards its own image in a glass, the light reflected from one of the candles by the sclerotica appeared upon its external margin, so as to define it distinctly by a bright line; and the image of the other candle was seen in the centre of the cornea. I then applied the double eye-glass, and the scale of the looking-glass, in the manner already described; but neither of them indicated any diminution of the distance, when the focal length of the eye was changed.

Another test, and a much more delicate one, was the application of the ring of a key at the external angle, when the eye was turned as much inwards as possible, and confined at the same time by a strong oval iron ring, pressed against it at the internal angle. The key was forced in as far as the sensibility of the integuments would admit, and was wedged, by a moderate pressure, between the eye and the bone. In this situation, the phantom caused by the pressure extended within the field of perfect vision, and was very accurately defined; nor did it, as I formerly imagined, by any means prevent a distinct perception of the objects actually seen in that direction; and a straight line coming within the field of this oval phantom, appeared somewhat inflected towards its centre; (Plate III. Fig. 14.) a distortion easily understood by considering the effect of the pressure on the form of the retina. Supposing now, the distance between the key and the iron ring to have been, as it really was, invariable, the elongation of the eye must have been either totally or very nearly prevented; and, instead of an

increase of the length of the eye's axis, the oval spot caused by the pressure would have spread over a space at least ten times as large as the most sensible part of the retina. But no such circumstance took place: the power of accommodation was as extensive as ever; and there was no perceptible change, either in the size or in the figure of the oval spot.

Again, since the rays which pass through the centre of the pupil, or rather the anterior vertex of the lens, may, as already observed, be considered as delineating the image; and, since the divergence of these rays with respect to each other, is but little affected by the refraction of the lens, they may still be said to diverge from the centre of the pupil; and the image of a given object on the retina must be very considerably enlarged, by the removal of the retina to a greater distance from the pupil and lens. (Cor. Prop. V*.) To ascertain the real magnitude of the image with accuracy, is not so easy as it at first sight appears; but, besides the experiment last related, which might be employed as an argument to this purpose, there are two other methods of estimating it. The first is too hazardous to be of much use; but, with proper precautions, it may be attempted. I fix my eye on a brass circle placed in the rays of the sun, and, after some time, remove it to the cancellated micrometer; then, changing the focus of my eye, while the micrometer remains at a given distance, I endeavour to discover whether there is any difference in the apparent magnitude of the spectrum on the scale; but I can discern none. I have not insisted on the attempt; especially as I have not been able to make the

* This Corollary should stand thus. "If a confused image be received on any given plane, it will be necessary, in order to determine its magnitude, to advert to the aperture admitting the rays. If the aperture be supposed to be infinitely small, it may be considered as a radiant point, in order to find the direction of the emergent rays."

spectrum distinct enough without inconvenience; and no light is sufficiently strong to cause a permanent impression on any part of the retina remote from the visual axis. I therefore had recourse to another experiment. I placed two candles so as exactly to answer to the extent of the termination of the optic nerve, and, marking accurately the point to which my eye was directed, I made the utmost change in its focal length; expecting that, if there were any elongation of the axis, the external candle would appear to recede outwards upon the visible space. (Plate III. Fig. 15.) But this did not happen; the apparent place of the obscure part was precisely the same as before. I will not undertake to say, that I could have observed a very minute difference either way: but I am persuaded, that I should have discovered an alteration of less than a tenth part of the whole.

It may be inquired if no change in the magnitude of the image is to be expected on any other supposition; and it will appear to be possible, that the changes of curvature may be so adapted, that the magnitude of the confused image may remain perfectly constant. Indeed, to calculate from the dimensions which we have hitherto used, it would be expected that the image should be diminished about one-sixtieth, by the utmost increase of the convexity of the lens. But the whole depends on the situation of the refracting surfaces, and the respective increase of their curvature, which, on account of the variable density of the lens, can scarcely be estimated with sufficient accuracy. Had the pupil been placed before the cornea, the magnitude of the image must, on any supposition, have been very variable: at present, this inconvenience is avoided by the situation of the pupil; so that we have here an additional instance of the perfection of this admirable organ.

From the experiments related, it appears to be highly improbable that any material change in the length of the axis actually takes place; and it is almost impossible to conceive by what power such a change could be effected. The straight muscles, with the adipose substance lying under them, would certainly, when acting independently of the socket, tend to flatten the eye: for, since their contraction would necessarily lessen the circumference or superficies of the mass that they contain, and round off all its prominences, their attachment about the nerve and the anterior part of the eye must therefore be brought nearer together. (Plate V. Fig. 21, 22.) Dr. Olbers compares the muscles and the eye to a cone, of which the sides are protruded, and would by contraction be brought into a straight line. But this would require a force to preserve the cornea as a fixed point, at a given distance from the origin of the muscles; a force which certainly does not exist. In the natural situation of the visual axis, the orbit being conical, the eye might be somewhat lengthened, although irregularly, by being forced further into it; but, when turned towards either side, the same action would rather shorten its axis; nor is there any thing about the human eye that could supply its place. In quadrupeds, the oblique muscles are wider than in man; and in many situations might assist in the effect. Indeed a portion of the orbicular muscle of the globe is attached so near to the nerve, that it might also co-operate in the action: and I have no reason to doubt the accuracy of Dr. Olbers, who states, that he effected a considerable elongation, by tying threads to the muscles, in the eyes of hogs and of calves; yet he does not say in what position the axis was fixed; and the flaccidity of the eye after death might render such a change very easy as

would be impossible in a living eye. Dr. OLBERS also mentions an observation of Professor WRISBERG, on the eye of a man whom he believed to be destitute of the power of accommodation in his life-time, and whom he found, after death, to have wanted one or more of the muscles: but this want of accommodation was not at all accurately ascertained. I measured, in the human eye, the distance of the attachment of the inferior oblique muscle from the insertion of the nerve: it was one-fifth of an inch; and from the centre of vision not a tenth of an inch; so that, although the oblique muscles do in some positions nearly form a part of a great circle round the eye, their action would be more fitted to flatten than to elongate it. We have therefore reason to agree with WINSLOW, in attributing to them the office of helping to support the eye on that side where the bones are most deficient: they seem also well calculated to prevent its being drawn too much backwards by the action of the straight muscles. And, even if there were no difficulty in supposing the muscles to elongate the eye in every position, yet at least some small difference would be expected in the extent of the change, when the eye is in different situations, at an interval of more than a right angle from each other; but the optometer shews that there is none.

Dr. HOSACK alleges that he was able, by making a pressure on the eye, to accommodate it to a nearer object:* it does not appear that he made use of very accurate means of ascertaining the fact; but, if such an effect took place, the cause must have been an inflection of the cornea.

It is unnecessary to dwell on the opinion which supposes a joint operation, of changes in the curvature of the cornea and

* Phil. Trans. for 1794. p. 212.

in the length of the axis. This opinion had derived very great respectability, from the most ingenious and elegant manner in which Dr. OLBERS had treated it, and from being the last result of the investigation of Mr. HOME and Mr. RAMSDEN. But either of the series of experiments which have been related, appears to be sufficient to confute it.

X. It now remains to inquire into the pretensions of the crystalline lens to the power of altering the focal length of the eye. The grand objection to the efficacy of a change of figure in the lens, was derived from the experiments in which those who have been deprived of it have appeared to possess the faculty of accommodation.

My friend Mr. WARE, convinced as he was of the neatness and accuracy of the experiments related in the Croonian Lecture for 1795, yet could not still help imagining, from the obvious advantage all his patients found, after the extraction of the lens, in using two kinds of spectacles, that there must, in such cases, be a deficiency in that faculty. This circumstance, combined with a consideration of the directions very judiciously given by Dr. PORTERFIELD, for ascertaining the point in question, first made me wish to repeat the experiments upon various individuals, and with the instrument which I have above described as an improvement of Dr. PORTERFIELD's optometer: and I must here acknowledge my great obligation to Mr. WARE, for the readiness and liberality with which he introduced me to such of his numerous patients as he thought most likely to furnish a satisfactory determination. It is unnecessary to enumerate every particular experiment; but the universal result is, contrary to the expectation with which I entered on the inquiry, that in an eye deprived of the crystalline lens, the

actual focal distance is totally unchangeable. This will appear from a selection of the most decisive observations.

1. Mr. R. can read at four inches and at six only, with the same glass. He saw the double lines meeting at three inches, and always at the same point; but the cornea was somewhat irregularly prominent, and his vision not very distinct; nor had I, at the time I saw him, a convenient apparatus.

I afterwards provided a small optometer, with a lens of less than two inches focus, adding a series of letters, not in alphabetical order, and projected into such a form as to be most legible at a small inclination. The excess of the magnifying power had the advantage of making the lines more divergent, and their crossing more conspicuous; and the letters served for more readily naming the distance of the intersection, and, at the same time, for judging of the extent of the power of distinguishing objects too near or too remote for perfect vision. (Plate V. Fig. 23.)

2. Mr. J. had not an eye very proper for the experiment; but he appeared to distinguish the letters at $2\frac{1}{2}$ inches, and at less than an inch. This at first persuaded me, that he must have a power of changing the focal distance: but I afterwards recollected that he had withdrawn his eye considerably, to look at the nearer letters, and had also partly closed his eyelids, no doubt contracting at the same time the aperture of the pupil; an action which, even in a perfect eye, always accompanies the change of focus. The slider was not applied.

3. Miss H. a young lady of about twenty, had a very narrow pupil, and I had not an opportunity of trying the small optometer: but, when she once saw an object double through the slits, no exertion could make it appear single at the same dis-

tance. She used for distant objects a glass of $4\frac{1}{2}$ inches focus; with this she could read as far off as 12 inches, and as near as five: for nearer objects she added another of equal focus, and could then read at 7 inches, and at $2\frac{1}{2}$.

4. HANSON, a carpenter, aged 63, had a cataract extracted a few years since from one eye: the pupil was clear and large, and he saw well to work with a lens of $2\frac{3}{8}$ inches focus; and could read at 8 and at 15 inches, but most conveniently at 11. With the same glass, the lines of the optometer appeared always to meet at 11 inches; but he could not perceive that they crossed, the line being too strong, and the intersection too distant. The experiment was afterwards repeated with the small optometer: he read the letters from 2 to 3 inches; but the intersection was always at $2\frac{1}{2}$ inches. He now fully understood the circumstances that were to be noticed, and saw the crossing with perfect distinctness: at one time, he said it was a tenth of an inch nearer; but I observed that he had removed his eye two or three tenths from the glass, a circumstance which accounted for this small difference.

5. Notwithstanding HANSON's age, I consider him as a very fair subject for the experiment. But a still more unexceptionable eye was that of Mrs. MABERLY. She is about 30, and had the crystalline of both eyes extracted a few years since, but sees best with her right. She walks without glasses; and, with the assistance of a lens of about four inches focus, can read and work with ease. She could distinguish the letters of the small optometer from an inch to $2\frac{1}{2}$ inches; but the intersection was invariably at the same point, about 19 tenths of an inch distant. A portion of the capsule is stretched across the pupil, and causes her to see remote objects double, when without her

glasses; nor can she, by any exertion, bring the two images nearer together, although the exertion makes them more distinct, no doubt by contracting the pupil. The experiment with the optometer was conducted, in the presence of Mr. WARE, with patience and perseverance; nor was any opinion given to make her report partial.

Considering the difficulty of finding an eye perfectly suitable for the experiments, these proofs may be deemed tolerably satisfactory. But, since one positive argument will counterbalance many negative ones, provided it be equally grounded on fact, it becomes necessary to inquire into the competency of the evidence employed to ascertain the power of accommodation attributed, in the Croonian Lecture for 1794, to the eye of BENJAMIN CLERK. And it appears, that the distinction long since very properly made by Dr. JURIN, between distinct vision and perfect vision, will readily explain away the whole of that evidence.

It is obvious that vision may be made distinct to any given extent, by means of an aperture sufficiently small, provided at the same time, that a sufficient quantity of light be left, while the refractive powers of the eye remain unchanged. And it is remarkable, that in those experiments, when the comparison with the perfect eye was made, the aperture of the imperfect eye only was very considerably reduced. BENJAMIN CLERK, with an aperture of $\frac{3}{40}$ of an inch, could read with the same glass at $1\frac{7}{8}$ inch, and at 7 inches.* With an equal aperture, I can read at $1\frac{1}{2}$ inch and at 30 inches: and I can retain the state of perfect relaxation, and read with the same aperture at $2\frac{1}{4}$ inches; and this is as great a difference as was observed in

* Phil. Trans. for 1795. p. 9.

BENJAMIN CLERK's eye. It is also a fact of no small importance, that Sir HENRY ENGLEFIELD was much astonished, as well as the other observers, at the accuracy with which the man's eye was adjusted to the same distance, in the repeated trials that were made with it.* This circumstance alone makes it highly probable, that its perfect vision was confined within very narrow limits.

Hitherto I have endeavoured to shew the inconveniences attending other suppositions, and to remove the objections to the opinion of an internal change of the figure of the lens. I shall now state two experiments, which, in the first place, come very near to a mathematical demonstration of the existence of such a change, and, in the second, explain in great measure its origin, and the manner in which it is effected.

I have already described the appearances of the imperfect image of a minute point at different distances from the eye, in a state of relaxation. For the present purpose, I will only repeat, that if the point is beyond the furthest focal distance of the eye, it assumes that appearance which is generally described by the name of a star, the central part being considerably the brightest. (Plate VI. Fig. 36—39.) But, when the focal distance of the eye is shortened, the imperfect image is of course enlarged; and, besides this necessary consequence, the light is also very differently distributed; the central part becomes faint, and the margin strongly illuminated, so as to have almost the appearance of an oval ring. (Fig. 41.) If I apply the slider of the optometer, the shadows of the slits, while the eye is relaxed, are perfectly straight, dividing the oval either way into parallel segments: (Fig 42, 44.) but, when the accom-

* Phil. Trans. for 1795. p. 8.

modation takes place, they immediately become curved, and the more so the further they are from the centre of the image, to which their concavity is directed. (Fig. 43, 45.) If the point be brought much within the focal distance, the change of the eye will increase the illumination of the centre, at the expense of the margin. The same appearances are equally observable, when the effect of the cornea is removed by immersion in water; and the only imaginable way of accounting for the diversity, is to suppose the central parts of the lens to acquire a greater degree of curvature than the marginal parts. If the refraction of the lens remained the same, it is absolutely impossible that any change of the distance of the retina should produce a curvature in those shadows, which, in the relaxed state of the eye, are found to be in all parts straight; and, that neither the form nor the relative situation of the cornea is concerned, appears from the application of water already mentioned.

The truth of this explanation is fully confirmed by the optometer. When I look through four narrow slits, without exertion, the lines always appear to meet in one point: but, when I make the intersection approach me, the two outer lines meet considerably beyond the inner ones, and the two lines of the same side cross each other at a still greater distance. (Plate V. Fig. 24.)

The experiment will not succeed with every eye; nor can it be expected that such an imperfection should be universal: but one case is sufficient to establish the argument, even if no other were found. I do not however doubt, that in those who have a large pupil, the aberration may be very frequently observable. In Dr. Wollaston's eye, the diversity of appearance is imperceptible; but Mr. König described the intersections exactly as

they appear to me, although he had received no hint of what I had observed. The lateral refraction is the most easily ascertained, by substituting for the slits a tapering piece of card, so as to cover all the central parts of the pupil, and thus determining the nearest crossing of the shadows transmitted through the marginal parts only. When the furthest intersection was at 38, I could bring it to 22 parts with two narrow slits; but with the tapered card only to 29. From these data we may determine pretty nearly, into what form the lens must be changed; supposing both the surfaces to undergo proportional alterations of curvature, and taking for granted the dimensions already laid down: for, from the lateral aberration thus given, we may find (by Prop. III.) the subtangents at about one-tenth of an inch from the axis; and the radius of curvature at each vertex, is already determined to be about 21 and 15 hundredths of an inch. Hence the anterior surface must be a portion of a hyperboloid, of which the greater axis is about 50; and the posterior surface will be nearly parabolical. In this manner the change will be effected, without any diminution of the transverse diameter of the lens. The elongation of its axis will not exceed the fiftieth of an inch; and, on the supposition with which we set out, the protrusion will be chiefly at the posterior vertex. The form of the lens thus changed will be nearly that of Plate V. Fig. 26; the relaxed state being nearly as represented in Fig. 25. Should, however, the rigidity of the internal parts, or any other considerations, render it convenient to suppose the anterior surface more changed, it would still have room, without interfering with the uvea; or it might even force the uvea a little forwards, without any visible alteration of the external appearance of the eye.

From this investigation of the change of the figure of the lens, it appears that the action which I formerly attributed to the external coats, cannot afford an explanation of the phenomenon. The necessary effect of such an action would be, to produce a figure approaching to that of an oblate spheroid; and, to say nothing of the inconvenience attending a diminution of the diameter of the lens, the lateral refraction would be much more increased than the central; nor would the slight change of density, at an equal distance from the axis, be at all equivalent to the increase of curvature: we must therefore suppose some different mode of action in the power producing the change. Now, whether we call the lens a muscle or not, it seems demonstrable, that such a change of figure takes place as can be produced by no external cause; and we may at least illustrate it by a comparison with the usual action of muscular fibres. A muscle never contracts, without at the same time swelling laterally, and it is of no consequence which of the effects we consider as primary. I was induced, by an occasional opacity, to give the name of membranous tendons to the radiations from the centre of the lens; but, on a more accurate examination, nothing really analogous to tendon can be discovered. And, if it were supposed that the parts next the axis were throughout of a tendinous, and therefore unchangeable nature, the contraction must be principally effected by the lateral parts of the fibres; so that the coats would become thicker towards the margin, by their contraction, while the general alteration of form would require them to be thinner; and there would be a contrariety in the actions of the various parts. But, if we compare the central parts of each surface to the belly of the muscle, there is no difficulty in

conceiving their thickness to be immediately increased, and to produce an immediate elongation of the axis, and an increase of the central curvature; while the lateral parts co-operate more or less, according to their distance from the centre, and in different individuals in somewhat different proportions. On this supposition, we have no longer any difficulty in attributing a power of change to the crystalline of fishes. M. PETIT, in a great number of observations, uniformly found the lens of fishes more or less flattened: but, even if it were not, a slight extension of the lateral part of the superficial fibres would allow those softer coats to become thicker at each vertex, and to form the whole lens into a spheroid somewhat oblong; and here, the lens being the only agent in refraction, a less alteration than in other animals would be sufficient. It is also worthy of inquiry, whether the state of contraction may not immediately add to the refractive power. According to the old experiment, by which Dr. GODDARD attempted to show that muscles become more dense as they contract, such an effect might naturally be expected That experiment is, however, very indecisive, and the opinion is indeed generally exploded, but perhaps too hastily; and whoever shall ascertain the existence or non-existence of such a condensation, will render essential service to physiology in general.

Dr. PEMBERTON, in the year 1719, first systematically discussed the opinion of the muscularity of the crystalline lens.* He referred to LEEUWENHOEK'S microscopical observations; but he so overwhelmed his subject with intricate calculations, that few have attempted to develope it: and he grounded the

* De Facultate Oculi qua ad diversas Rerum distantias se accommodat. L. B. 1719. Ap. Hall. Disp. Anat. IV. p. 301.

whole on an experiment borrowed from BARROW, which with me has totally failed; and I cannot but agree with Dr. OLBERS in the remark, that it is easier to confute him than to understand him. He argued for a partial change of the figure of the lens; and perhaps the opinion was more just than the reasons adduced for its support. LOBE', or rather ALBINUS,* decidedly favours a similar theory; and suggests the analogy of the lens to the muscular parts of pellucid animals, in which even the best microscopes can discover no fibres. CAMPER also mentions the hypothesis with considerable approbation.† Professor REIL published, in 1793, a Dissertation on the Structure of the Lens; and, in a subsequent paper, annexed to the translation of my former Essay in Professor GREN's Journal, § he discussed the question of its muscularity. I regret that I have not now an opportunity of referring to this publication; but I do not recollect that Professor REIL's objections are different from those which I have already noticed.

Considering the sympathy of the crystalline lens with the uvea, and the delicate nature of the change of its figure, there is little reason to expect that any artificial stimulus would be more successful in exciting a contractive action in the lens, than it has hitherto been in the uvea; much less would that contraction be visible without art. Soon after Mr. HUNTER's death, I pursued the experiment which he had suggested, for ascertaining how far such a contraction might be observable. My apparatus (Plate V. Fig. 27.) was executed by Mr. JONES. It consisted of a wooden vessel blacked within, which was to be

* De quibusdam Oculi Partibus, L. B. 1746. Ap. Hall. Disp. Anat. IV. p. 301.

† De Oculo Humano. L. B. 1742. Ap. Hall. Disp. Anat. VII. 2. p. 108, 109.

§ 1794. p. 352, 354.

filled with cool, and then with warmer water: a plane speculum was placed under it; a perforation in the bottom was filled with a plate of glass; proper rings were fixed for the reception of the lens, or of the whole eye, and also wires for transmitting electricity: above these, a piece of ground and painted glass, for receiving the image, was supported by a bracket, which moved by a pivot, in connection with a scale divided into fiftieths of an inch. With this apparatus I made some experiments, assisted by Mr. Wilkinson, whose residence was near a slaughter-house: but we could obtain, by this method, no satisfactory evidence of the change; nor was our expectation much disappointed. I understand also, that another member of this Society was equally unsuccessful, in attempting to produce a conspicuous change in the lens by electricity.

XI. In man and in the most common quadrupeds, the structure of the lens is nearly similar. The number of radiations is of little consequence; but I find that in the human crystalline there are ten on each side, (Plate VI. Fig. 46.) not three, as I once, from a hasty observation, concluded.* Those who find any difficulty in discovering the fibres, must have a sight very ill adapted to microscopical researches. I have laboured with the most obstinate perseverance to trace nerves into the lens, and I have sometimes imagined that I had succeeded; but I cannot positively go further than to state my full conviction of their existence, and of the precipitancy of those who have absolutely denied it. The long nerves, which are very conspicuous between the choroid and sclerotic coats, divide each into two, three, or more branches, at the spot where the ciliary zone begins, and seem indeed to furnish the choroid with some fine

* De Corp. Hum. Vir. Cons. p. 68.

filaments at the same place. The branches often re-unite, with a slight protuberance, that scarcely deserves the name of a ganglion: here they are tied down, and mixed with the hard whitish-brown membrane that covers the compact spongy substance, in which the vessels of the ciliary processes anastomose and subdivide. (Plate VI. Fig. 47.) The quantity of the nerves which proceeds to the iris, appears to be considerably smaller than that which arrives at the place of division: hence there can be little doubt that the division is calculated to supply the lens with some minute branches; and it is not improbable, from the appearance of the parts, that some fibres may pass to the cornea; although it might more naturally be expected, that the tunica conjunctiva would be supplied from without. But the subdivisions which probably pass to the lens, enter immediately into a mixture of ligamentous substance and of a tough brownish membrane; and I have not hitherto been able to develope them. Perhaps animals may be found in which this substance is of a different nature; and I do not despair that, with the assistance of injections, for more readily distinguishing the blood vessels, it may still be possible to trace them in quadrupeds. Our inability to discover them, is scarcely an argument against their existence: they must naturally be delicate and transparent; and we have an instance, in the cornea, of considerable sensibility, where no nerve has yet been traced. The capsule adheres to the ciliary substance, and the lens to the capsule, principally in two or three points; but I confess, I have not been able to observe that these points are exactly opposite to the trunks of nerves; so that, probably, the adhesion is chiefly caused by those vessels which are sometimes seen passing to the capsule in injected eyes. We may, however,

discover ramifications from some of these points, upon and within the substance of the lens, (Plate VI. Fig. 48.) generally following a direction near to that of the fibres, and sometimes proceeding from a point opposite to one of the radiating lines of the same surface. But the principal vessels of the lens appear to be derived from the central artery, by two or three branches at some little distance from the posterior vertex; which I conceive to be the cause of the frequent adhesion of a portion of a cataract to the capsule, about this point: they follow nearly the course of the radiations, and then of the fibres; but there is often a superficial subdivision of one of the radii, at the spot where one of them enters. The vessels coming from the choroid appear principally to supply a substance, hitherto unobserved, which fills up the marginal part of the capsule of the crystalline, in the form of a thin zone, and makes a slight elevation, visible even through the capsule. (Fig. 49—51.) It consists of coarser fibres than the lens, but in a direction nearly similar; they are often intermixed with small globules. In some animals, the margin of the zone is crenated, especially behind, where it is shorter: this is observable in the partridge; and, in the same bird, the whole surface of the lens is seen to be covered with points, or rather globules, arranged in regular lines, (Plate VII. Fig. 52.) so as to have somewhat the appearance of a honeycomb, but towards the vertex less uniformly disposed. This regularity is a sufficient proof that there could be no optical deception in the appearance; although it requires a good microscope to discover it distinctly: but the zone may be easily peeled off under water, and hardened in spirits. Its use is uncertain; but it may possibly secrete the liquid of the crystalline; and it as much deserves the

name of a gland, as the greater part of the substances usually so denominated. In peeling it off, I have very distinctly observed ramifications, which were passing through it into the lens; (Plate VI. Fig. 50.) and indeed it is not at all difficult to detect the vessels connecting the margin of the lens with its capsule; and it is surprising that M. PETIT should have doubted of their existence. I have not yet clearly discerned this crystalline gland in the human eye; but I infer the existence of something similar to the globules, from the spotted appearance of the image of a lucid point already mentioned; for which I can no otherwise account, than by attributing it to a derangement of these particles, produced by the external force, and to an unequal impression made by them on the surface of the lens.

In birds and in fishes, the fibres of the crystalline radiate equally, becoming finer as they approach the vertex, till they are lost in a uniform substance, of the same degree of firmness, which appears to be perforated in the centre by a blood vessel. (Plate VII. Fig. 53.) In quadrupeds, the fibres at their angular meeting are certainly not continued, as LEEUWENHOEK imagined, across the line of division; but there does not appear to be any dissimilar substance interposed between them, except that very minute trunks of vessels often mark that line. But, since the whole mass of the lens, as far as it is moveable, is probably endued with a power of changing its figure, there is no need of any strength of union, or place of attachment, for the fibres, since the motion meets with little or no resistance. Every common muscle, as soon as its contraction ceases, returns to its natural form, even without the assistance of an antagonist; and the lens itself, when taken out of the eye, in its capsule,

has elasticity enough to reassume its proper figure, on the removal of a force that has compressed it. The capsule is highly elastic; and, since it is laterally fixed to the ciliary zone, it must co-operate in restoring the lens to its flattest form. If it be inquired, why the lens is not capable of becoming less convex, as well as more so, it may be answered, that the lateral parts have probably little contractive power; and, if they had more, they would have no room to increase the size of the disc, which they must do, in order to shorten the axis; and the parts about the axis have no fibres so arranged as to shorten it by their own contraction.

I consider myself as being partly repaid for the labour lost in search of the nerves of the lens, by having acquired a more accurate conception of the nature and situation of the ciliary substance. It had already been observed, that in the hare and in the wolf, the ciliary processes are not attached to the capsule of the lens; and if by the ciliary processes we understand those filaments which are seen detached after tearing away the capsule, and consist of ramifying vessels, the observation is equally true of the common quadrupeds, and I will venture to say, of the human eye.* Perhaps this remark has been made by others, but the circumstance is not generally understood. It is so difficult to obtain a distinct view of these bodies, undisturbed, that I am partly indebted to accident, for having been undeceived respecting them: but, having once made the observation, I have learnt to show it in an unquestionable manner. I remove the posterior hemisphere of the sclerotica, or somewhat more, and also as much as possible of the vitreous humour, introduce the point of a pair of scissors

* Vid. Hall. Physiol. V. p. 432. et DUVERNEY, ibi citat.

into the capsule, turn out the lens, and cut off the greater part of the posterior portion of the capsule, and of the rest of the vitreous humour. I next dissect the choroid and uvea from the sclerotica; and, dividing the anterior part of the capsule into segments from its centre, I turn them back upon the ciliary zone. The ciliary processes then appear, covered with their pigment, and perfectly distinct both from the capsule and from the uvea; (Plate VII. Fig. 54.) and the surface of the capsule is seen shining, and evidently natural, close to the base of these substances. I do not deny that the separation between the uvea and the processes, extends somewhat further back than the separation between the processes and the capsule; but the difference is inconsiderable, and, in the calf, does not amount to above half the length of the detached part. The appearance of the processes is wholly irreconcileable with muscularity; and their being considered as muscles attached to the capsule, is therefore doubly inadmissible. Their lateral union with the capsule, commences at the base of their posterior smooth surface, and is continued nearly to the point where they are more intimately united with the termination of the uvea; so that, however this portion of the base of the processes were disposed to contract, it would be much too short to produce any sensible effect. What their use may be, cannot easily be determined: if it were necessary to have any peculiar organs for secretion, we might call them glands, for the percolation of the aqueous humour; but there is no reason to think them requisite for this purpose.

The marsupium nigrum of birds, and the horse-shoe-like appearance of the choroid of fishes, are two substances which have sometimes, with equal injustice, been termed muscular. All the apparent fibres of the marsupium nigrum are, as

HALLER had very truly asserted, merely duplicatures of a membrane, which, when its ends are cut off, may easily be unfolded under the microscope, with the assistance of a fine hair pencil, so as to leave no longer any suspicion of a muscular texture. The experiment related by Mr. HOME,* can scarcely be deemed a very strong argument for attributing to this substance a faculty which its appearance so little authorises us to expect in it. The red substance in the choroid of fishes, (Plate VII. Fig. 55.) is more capable of deceiving the observer; its colour gives it some little pretension, and I began to examine it with a prepossession in favour of its muscular nature. But, when we recollect the general colour of the muscles of fishes, the consideration of its redness will no longer have any weight. Stripped of the membrane which loosely covers its internal surface, (Fig. 56.) it seems to have transverse divisions, somewhat resembling those of muscles, and to terminate in a manner somewhat similar; (Fig. 57.) but, when viewed in a microscope, the transverse divisions appear to be cracks, and the whole mass is evidently of a uniform texture, without the least fibrous appearance; and, if a particle of any kind of muscle is compared with it, the contrast becomes very striking. Besides, it is fixed down, throughout its extent, to the posterior lamina of the choroid, and has no attachment capable of directing its effect; to say nothing of the difficulty of conceiving what that effect could be. Its use must remain, in common with that of many other parts of the animal frame, entirely concealed from our curiosity.

The bony scales of the eyes of birds, which were long ago described in the Philosophical Transactions by Mr. RANBY,†

* Phil. Trans. for 1796. p. 18.

† Phil. Trans. Vol. XXXIII. p. 223. Abr. Vol. VII. p. 435.

and by Mr. WARREN *, afterwards in two excellent Memoirs of M. PETIT on the eye of the turkey and of the owl, † and lately by Mr. PIERCE SMITH, ‡ and Mr. HOME, § can, on any supposition, have but little concern in the accommodation of the eye to different distances: they rather seem to be necessary for the protection of that organ, large and prominent as it is, and unsupported by any strength in the orbit, against the various accidents to which the mode of life and rapid motion of those animals must expose it; and they are much less liable to fracture than an entire bony ring of the same thickness would have been. The marsupium nigrum appears to be intended to assist in giving strength to the eye, to prevent any change in the place of the lens by external force: it is so situated as to intercept but little light, and that little is principally what would have fallen on the insertion of the optic nerve; and it seems to be too firmly tied to the lens, even to admit any considerable elongation of the axis of the eye, although it certainly would not impede a protrusion of the cornea.

With respect to the eyes of insects, an observation of POUPART deserves to be repeated here. He remarks, that the eye of the libellula is hollow; that it communicates with an air-vessel placed longitudinally in the trunk of the body; and that it is capable of being inflated from this cavity: he supposes that the insect is provided with this apparatus, in order for the accommodation of its eye to the perception of objects at different distances. || I have not yet had an opportunity of examining

* Phil. Trans. Vol. XXXIV. p. 113. Abr. Vol. VII. p. 437.

† Mem. de l'Acad. 1735. p. 163. 1736, p. 166. Ed. Amst.

‡ Phil. Trans. for 1795. p. 263. § Phil. Trans. for 1796. p. 14.

|| Phil. Trans. Vol. XXII. p. 673. Abr. II. p. 762.

the eye of the libellula; but there is no difficulty in supposing that the means of producing the change of the refractive powers of the eye, may be, in different classes of animals, as diversified as their habits, and the general conformation of their organs.

I beg leave to correct here an observation in my former paper, relative to the faint lateral radiations, which I supposed to proceed from the margin of the iris.* I find, on further examination, that they are occasioned by reflections from the eyelashes.

XII. I shall now finally recapitulate the principal objects and results of the investigation which I have taken the liberty of detailing so fully to the Royal Society. First, the determination of the refractive power of a variable medium, and its application to the constitution of the crystalline lens. Secondly, the construction of an instrument for ascertaining, upon inspection, the exact focal distance of every eye, and the remedy for its imperfections. Thirdly, to show the accurate adjustment of every part of the eye, for seeing with distinctness the greatest possible extent of objects at the same instant. Fourthly, to measure the collective dispersion of coloured rays in the eye. Fifthly, by immerging the eye in water, to demonstrate that its accommodation does not depend on any change in the curvature of the cornea. Sixthly, by confining the eye at the extremities of its axis, to prove that no material alteration of its length can take place. Seventhly, to examine what inference can be drawn from the experiments hitherto made on persons deprived of the lens; to pursue the inquiry, on the principles suggested by Dr. PORTERFIELD; and to confirm his opinion of the utter inabi-

* Phil. Trans. for 1793. p. 178.

lity of such persons to change the refractive state of the organ. Eighthly, to deduce, from the aberration of the lateral rays, a decisive argument in favour of a change in the figure of the crystalline; to ascertain, from the quantity of this aberration, the form into which the lens appears to be thrown in my own eye, and the mode by which the change must be produced in that of every other person. And I flatter myself, that I shall not be deemed too precipitate, in denominating this series of experiments satisfactorily demonstrative.

CORRECTIONS.

Page 28, line 11, Prop. III. *after e, insert* the base being unity.

Page 30, line 8, Cor. 10. *for n t u, read n t t*; line 9, *for* product &c. *read* square of the cosine of incidence.

Page 31, line 5, Cor. 11. *for* $1 + u^2 - 2u^4$, *read* $2muu$.

Page 31. Prop. V. Cor. See the note in p. 60.

Page 33. Prop. VIII. By a mistake of a sign, the eighth proposition is rendered erroneous; no use having been made of that proposition, it has been inserted without proper revision. It ought to stand thus, with its demonstration:

Proposition VIII. Problem.

To find the path of a ray of light falling obliquely on a sphere, of a refractive density varying as any power of the distance from the centre.

The refractive density, in the sense of these propositions, varies as the ratio of the sines, and as the velocity of light in the medium. (Schol. 2. Prop. I.) Let the velocity at the distance x be $x^{-\frac{1}{r}}$; then, considering the refractive force as a species of attraction, we have, in Prop. 41. l. 1. Princip. $\sqrt{ABFD} = x^{-\frac{1}{r}}$, $Q = s$, the sine of incidence, the radius being unity, $Z = s\,x^{-1}$, $Dc = \dfrac{s}{2\,x\,x\sqrt{x^{-\frac{2}{r}} - s^2 x^{-2}}}$ $= \frac{1}{2} s\,x^{\frac{1}{r}-2} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\,\Big|^{-\frac{1}{2}}$, and the fluxion of the area described by the radius $= -\frac{1}{2} s\,x^{\frac{1}{r}-2}\,\dot{x} \cdot \overline{1 - s^2 x^{\frac{2}{r}-2}}\,\Big|^{-\frac{1}{2}}$. Let the sine of the inclination to the radius

at each point be called y; then $y = s\,x^{\frac{1}{r}-1}$, $\dot{y} = \frac{1-r}{r}\,s\,x^{\frac{1}{r}-2}\,\dot{x}$, and the fluxion of the area $= \frac{r}{2r-2}\,\dot{y}\,.\,\overline{1-yy}|^{-\frac{1}{2}}$, of which the fluent is $\frac{r}{2r-2}\,Y$, y being the sine of the arc Y; and the angle corresponding is $\frac{r}{r-1}\,Y$. The value of that angle being found for any two values of x or y, the difference is the intervening angle described by the radius. This angle is therefore always to the difference of the inclinations as r to $r-1$, and the deviation is to that difference as 1 to $r-1$.

Corollary. Hence, in the passage to the apsis, and the return to the surface, the deviation is always proportionate to the arc cut off by the incident ray produced: therefore such a sphere could never collect parallel rays to any focus, the lateral density being too small towards the surface.

Page 33, line 20, *for* but the two last &c. *read* the seventh may either be deduced from the eighth, or may be demonstrated independently of it.

Page 42, line 18, *after* internally, *insert* Or, if a lens of equal mean dimensions, and equal focal length, with the crystalline, be supposed to consist of two segments of the external portion of such a sphere, the refractive density at the centre of this lens must be as 18 to 17.

Page 47, line 12, *for* calculated &c. *read* estimated by means of the eighth proposition; and probably.

Page 53, line 24, *for* 24, *read* 21; line 25, *for* 17, *read* 15.

Page 61, line 21, *for* sixtieth, *read* fortieth.

EXPLANATION OF THE FIGURES.

Plate II. Fig. 1. See Page 28. Prop. III.

Fig. 2. See Page 28. Prop. IV.

Fig. 3. See Page 31. Prop. V.

Fig. 4—6. Relating to the optometer. See Page 34.

Plate III. Fig. 7. The form of the ends of the optometer, when made of card. The apertures in the shoulders are for holding a lens: the square ends turn under, and are fastened together.

Fig. 8. The scale of the optometer. The middle line is divided, from the lower end, into inches. The next column shows the number of a concave lens requisite for a short-sighted eye; by looking through the slider and observing the number opposite to which the intersection appears when most remote. By observing the place of apparent intersection when nearest, the number requisite will be found in the other column, provided that the eye have the average power of accommodation. At the other end, the middle line is graduated for extending the scale of inches by means of a lens four inches in focus; the negative numbers implying that such rays as proceed from them are made to converge towards a point on the other side of the lens. The other column shows the focal length of convex glasses required by those eyes to which the intersection appears, when nearest, opposite the respective places of the numbers.

Fig. 9. A side view of the optometer, half its size.

Fig. 10. The appearance of the lines through the slider.

Fig. 11. Method of measuring the magnitude of an image on the retina. See Page 48.

Fig. 12. Diagonal scale drawn on a looking-glass.

Fig. 13. The method of applying a lens with water to the cornea.

Fig. 14. The appearance of a spectrum occasioned by pressure; and the inflection of straight lines seen within the limits of the spectrum.

Fig. 15. An illustration of the enlargement of the image, which would be the consequence of an elongation of the eye: the images of the candles which, in one instance, fall on the insertion of the nerve, falling, in the other instance, beyond it.

Plate IV. Fig. 16. The successive forms of the image of a large distant object, as it would be delineated by each refractive surface in the eye; to show how that form at last coincides with the retina. E G is the distance between the foci of horizontal and vertical rays in my eye.

Plate V. Fig. 17. Vertical section of my right eye, seen from without; twice the natural size.

Fig. 18. Horizontal section, seen from above.

Fig. 19. Front view of my left eye when the pupil is contracted; of the natural size.

Fig. 20. The same view when the pupil is dilated.

Fig. 21. Outline of the eye and its straight muscles when at rest.

Fig. 22. Change of figure which would be the consequence of the action of those muscles upon the eye, and upon the adipose substance behind it.

Fig. 23. Scale of the small optometer.

Fig. 24. Appearance of four images of a line seen by my eye when its focus is shortest.

Fig. 25. Outline of the lens when relaxed; from a comparison of M. Petit's measures with the phenomena of my own eye, and on the supposition that it is found in a relaxed state after death.

Fig. 26. Outline of the lens sufficiently changed to produce the shortest focal distance.

Fig. 27. Apparatus for ascertaining the focal length of the lens in water.

Plate VI. Fig. 28. Various forms of the image depicted by a cylindrical pencil of rays obliquely refracted by a spherical surface, when received on planes at distances progressively greater.

Fig. 29. Image of a minute lucid object held very near to my eye.

Fig. 30. The same appearance when the eye has been rubbed.

Fig. 31—37. Different forms of the image of a lucid point at greater and greater distances; the most perfect focus being like Fig. 33, but much smaller.

Fig. 38. Image of a very remote point seen by my right eye.

Fig. 39. Image of a remote point seen by my left eye; being more obtuse at one end, probably from a less obliquity of the posterior surface of the crystalline lens.

Fig. 40. Combination of two figures similar to the fifth variety of Fig. 28; to imitate Fig. 38.

Fig. 41. Appearance of a distant lucid point when the eye is adapted to a very near object.

Fig, 42, 44. Shadow of parallel wires in the image of a distant point, when the eye is relaxed.

Fig. 43, 45. The same shadows rendered curved by a change in the figure of the crystalline lens.

Fig. 46. The order of the fibres of the human crystalline.

Fig. 47. The division of the nerves at the ciliary zone; the sclerotica being removed. One of the nerves of the uvea is seen passing forwards and subdividing. From the calf.

Fig. 48. Ramifications from the margin of the crystalline lens.

Fig. 49. The zone of the crystalline faintly seen through the capsule.

Fig. 50. The zone raised from its situation, with the ramifications passing through it into the lens.

Fig. 51. The zone of the crystalline detached.

Plate VII. Fig. 52. The crenated zone, and the globules regularly arranged on the crystalline of the partridge.

Fig. 53. The order of the fibres in the lens of birds and fishes.

Fig. 54. The segments of the capsule of the crystalline turned back, to show the detached ciliary processes. From the calf.

Fig. 55. Part of the choroid of the cod-fish, with its red substance. The central artery hangs loose from the insertion of the nerve.

Fig. 56. The membrane covering this substance internally, raised by the blow-pipe.

Fig. 57. The appearance of the red substance, after the removal of the membrane.

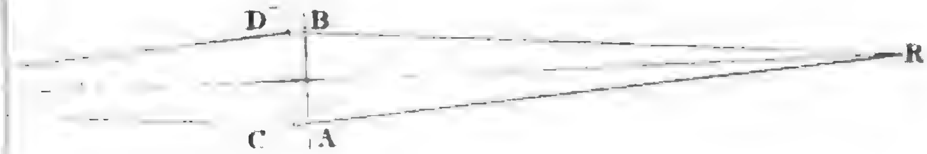


Fig. 6.

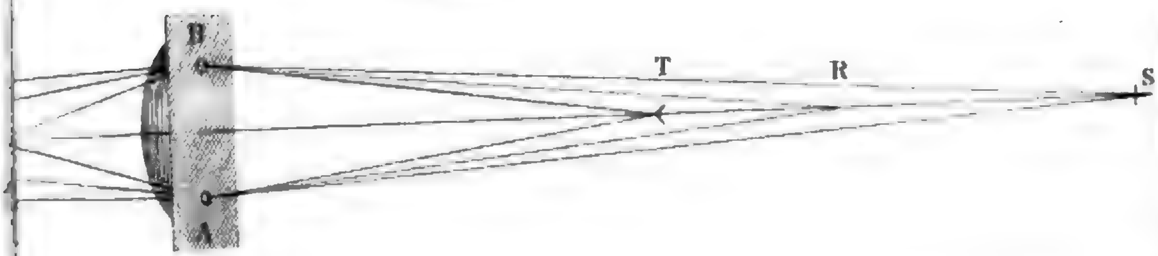


Fig. 7.

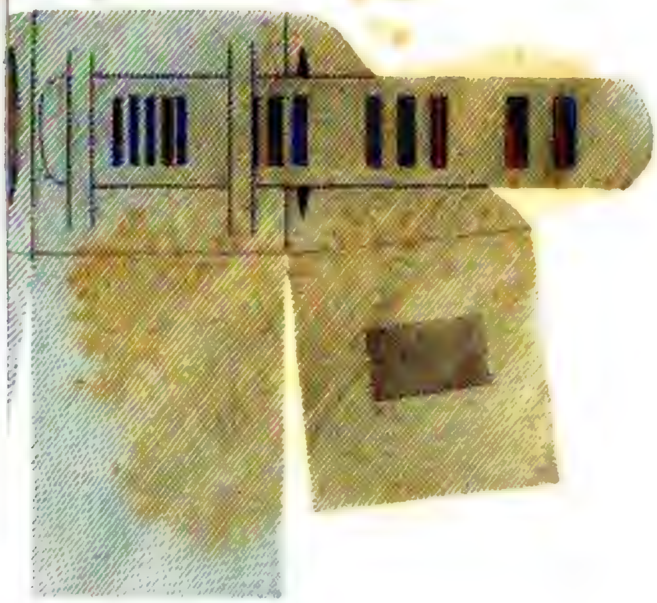
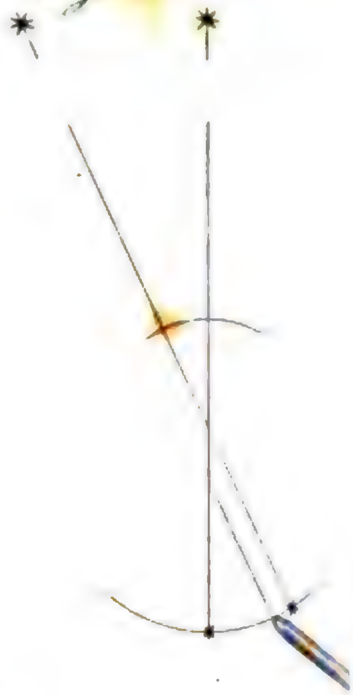


Fig. 11.



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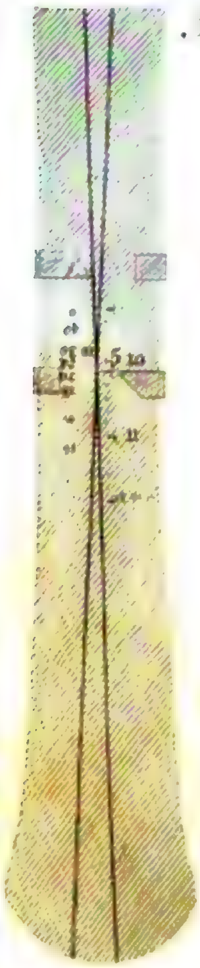


Fig. 12.

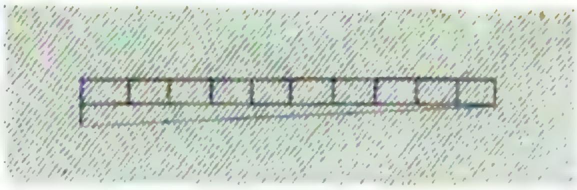
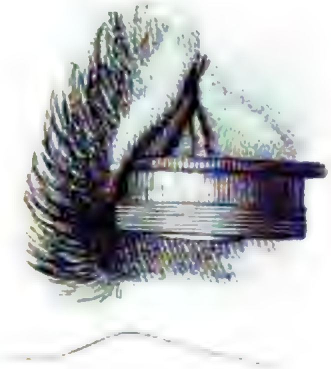


Fig. 13.



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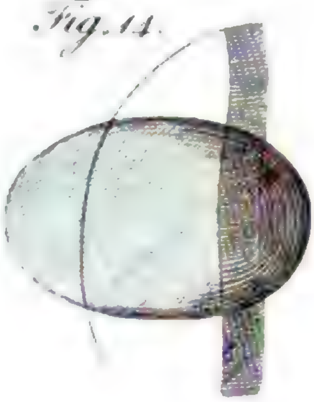


Fig. 15.

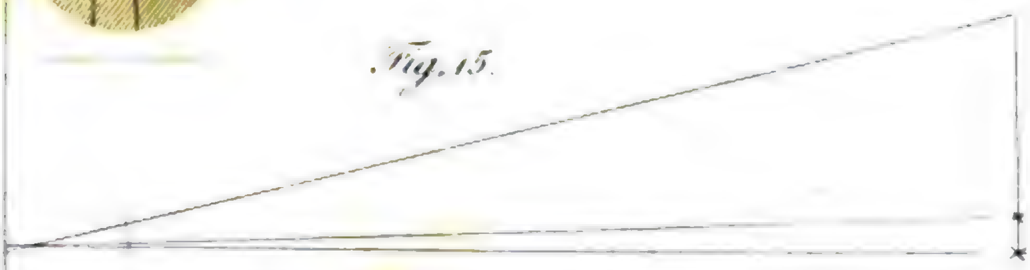


Fig. 16.

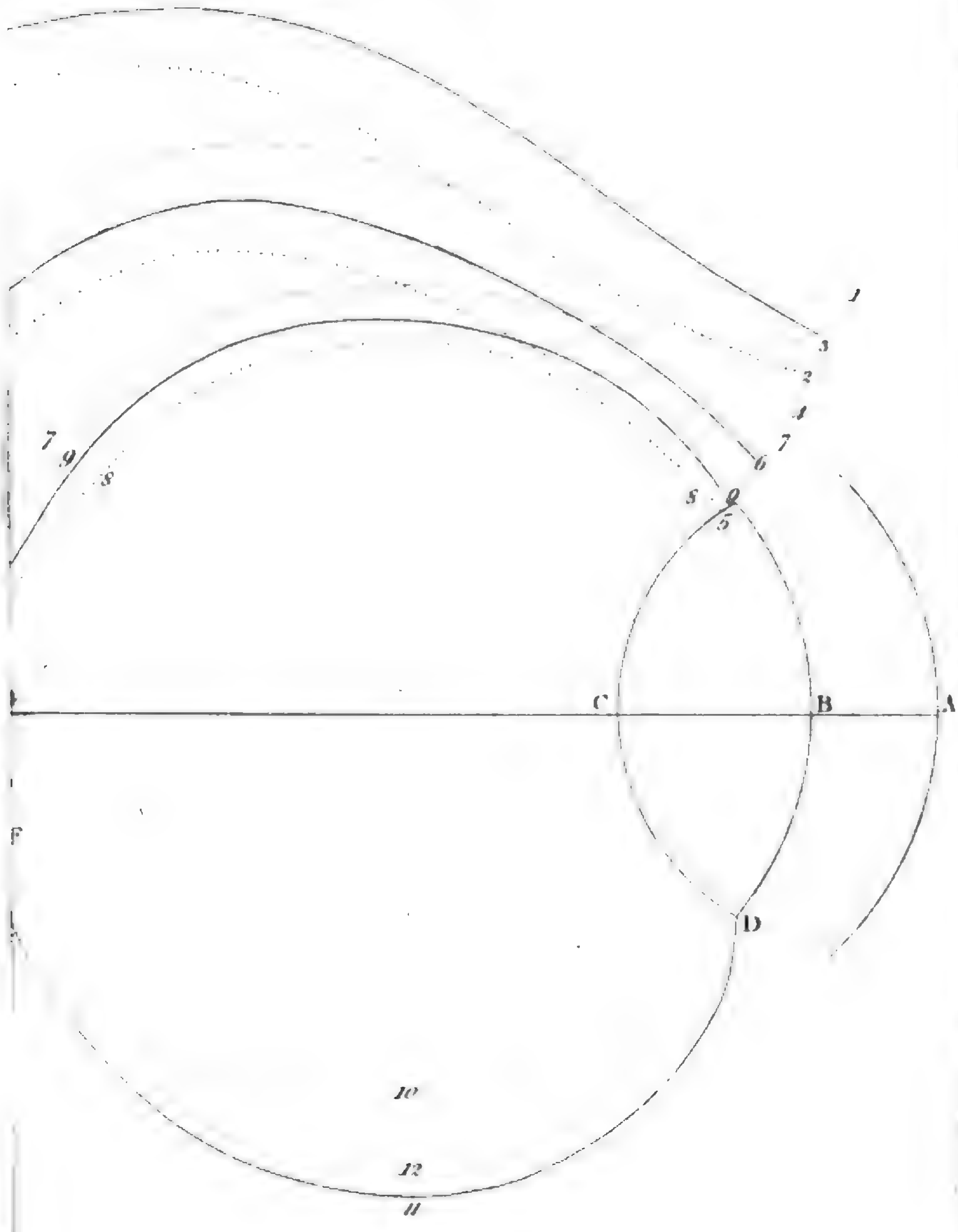


Fig. 18.

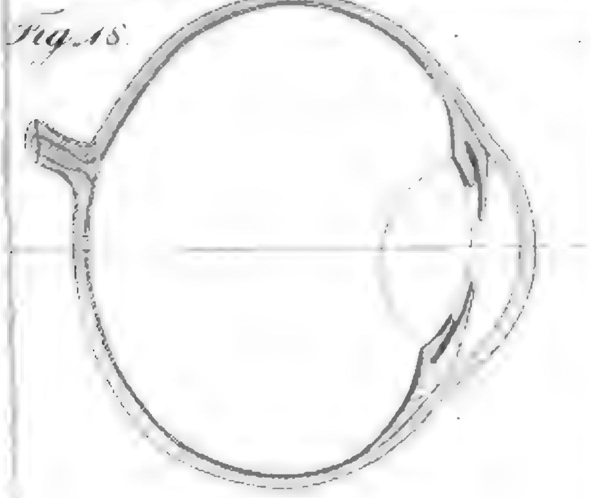


Fig. 19

Fig. 20

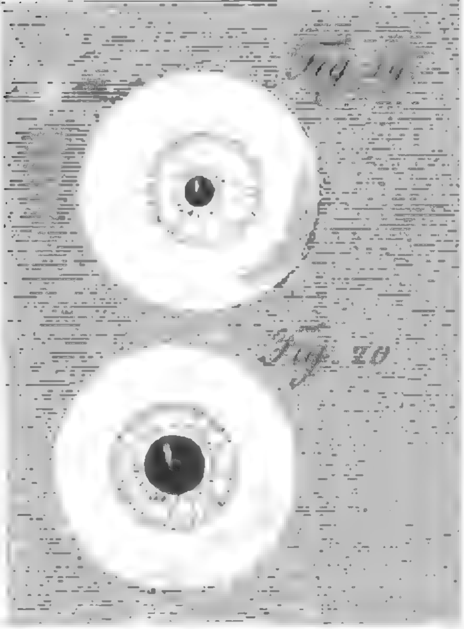
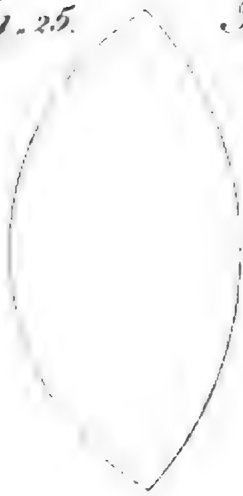


Fig. 22.

Fig. 25.



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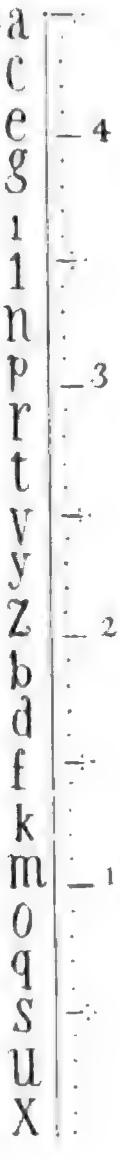

Fig. 24.

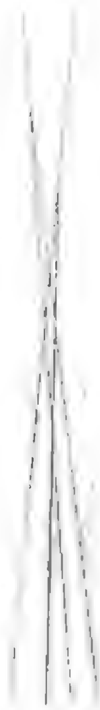

Fig. 26.

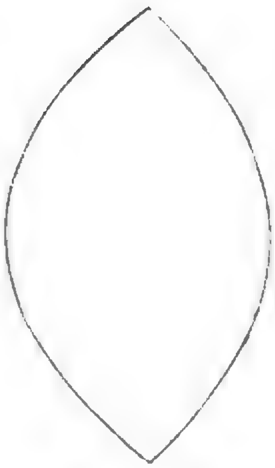


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Fig. 17.
Fig. 18.
Fig. 50.
Fig. 51.

Fig. 53.

Fig. 55.

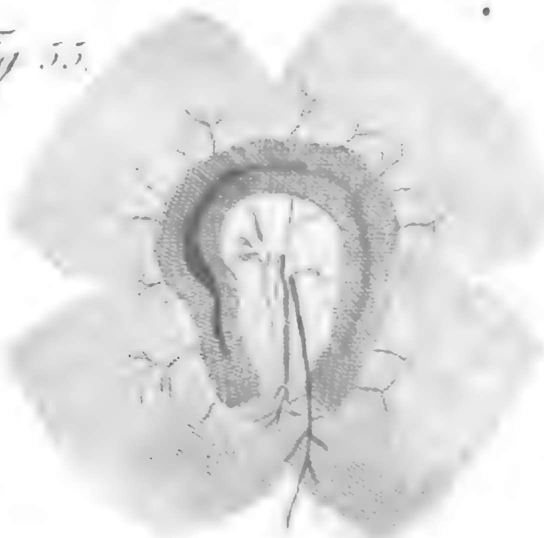


Fig. 57.

Basire sc.

III. *On the necessary Truth of certain Conclusions obtained by Means of imaginary Quantities. By* Robert Woodhouse, *A. M. Fellow of Caius College. Communicated by the Rev.* S. Vince, *A. M. Plumian Professor of Astronomy in the University of Cambridge.*

Read January 8, 1801.

AMONGST the various objections urged against mathematical science, few oppose its evidence and logical accuracy; and, since its demonstrations have been acknowledged to proceed by a series of the strictest inferences, from the most evident principles, the study of abstract science has generally been deemed peculiarly proper to habituate the mind to just reasoning. But of late, the dissentions of mathematicians have subjected to doubt, even this "collateral and intervenient use;" for, not only has the mode of applying analysis to physical objects been controverted, but certain parts of the pure mathematics have become the subject of dispute. Much has been heard of the science of quantity being vitiated with jargon, absurdity, and mystery, and perplexed with paradox and contradiction; so that, from the very complaints of the patrons of mathematics, its opponents may derive their most potent arguments, and abundant matter for triumphant invective.

The introduction of impossible quantities, is assigned as a great and primary cause of the evils under which mathematical science labours. During the operation of these quantities,

it is said, all just reasoning is suspended, and the mind is bewildered by exhibitions that resemble the juggling tricks of mechanical dexterity.

The arguments that seem to render all operations performed with impossible quantities unintelligible, may be included under the following statement. Algebra is a species of short-hand writing; a language, or system of characters or signs, invented for the purpose of facilitating the comparison and combination of ideas. Now all demonstration by signs, must ultimately rest on observations made on individual objects; and all the varieties of the transformation and combination of signs, except what are arbitrary and conventional, must be regulated by properties observed to belong to the things of which the signs are the representatives. Demonstration by signs is shewn to be true, by referring to the individual things the signs represent; and is shewn to be general, by remarking that the operation is the same, whatever is the thing signified, or, in other words, that the operation is independent of the things signified. Yet, against this statement, from the very concessions of the mathematicians that have opposed the use of impossible quantities, is to be derived a powerful argument, an argument sufficiently satisfactory to the mind, that operations with impossible quantities are really regulated by the rules of a logic equally just with the logic of possible quantities. It is conceded, and mentioned as a paradox, that the conclusions obtained by the aid of imaginary quantities are most true and certain. Now, if operations with any characters or signs lead to just conclusions, such operations must be true by virtue of some principle or other; and the objections against imaginary quantities, ought to be diverted upon the unsatisfactory explanation given of their

nature and uses. It would indeed be a singular paradox, or a rare felicity, if truth, not always attained by meditation, should unexpectedly result from un-ideal operations conducted without principle, purpose, or regularity.

The paradox, that a process in which no idea is introduced conducts to truth, and that operations by unintelligible characters lead to certain and just conclusions, has been expressly treated in a paper presented to the Royal Society. The ingenious author, confining his enquiry concerning impossible quantities to their use in calculating the values of sines, cosines, &c. has attempted to shew, that operations with such quantities are true, on the principle of analogy. He is of opinion, that, "The operations performed with imaginary chatacters, though destitute of meaning themselves, are yet notes of reference to others which are significant. They point out indirectly a method of demonstrating a certain property of the hyperbola, and then leave us to conclude from analogy, that the same property belongs also to the circle. All that we are assured of by the imaginary investigation is, that its conclusion may, with all the strictness of mathematical reasoning, be proved of the hyperbola; but if from thence we would transfer that conclusion to the circle, it must be in consequence of the principle just now mentioned. The investigation therefore resolves itself ultimately into an argument from analogy; and, after the strictest examination, will be found without any other claim to the evidence of demonstration." By this explanation, the operations of imaginary quantities, before disorderly and confused, assume some appearance of purpose and regularity; and the assent of the mind, if not compelled by certain proof, is at least solicited by probable arguments. But, to mathe-

maticians, who, in questions of abstract science, profess never to rest contented with "a rational faith and moral persuasion," the principle of explanation just adduced must needs be unsatisfactory; for, whatever extension of meaning be allowed to the term analogy, still this is certain, that a proof by analogy is inferior to strict demonstration. What is it that determines the nature of this analogy? Or how can its several coincidences, interruptions, and limitations be ascertained, except by separate and direct investigations of the properties of the circle and hyperbola? If the analogy between the two curves depends on investigation, and is limited thereby, then all operations with imaginary expressions are perfectly nugatory; since we are not warranted to adopt a single conclusion obtained by their aid, except such conclusion be verified by a distinct and rigorous demonstration.

The author of the principle of analogy allows that it is imperfect; and I perceive no sure method of ascertaining the restrictions to which it is subject, except by the forms that result from actual investigation.

To shew that the principle of analogy ought to be abandoned, and a more natural and satisfactory one sought for, an argument may be used, similar to the one employed against those who maintain operations by imaginary symbols to be perfectly unintelligible; that, since arguments have been invented, which, if they do not satisfy, yet afford the mind a glimpse and indistinct perception of the reason why certain processes lead to truth, it may be presumed possible to convert such probable arguments into certain proofs, and to discipline a vague, perilous, and irregular analogy, into a strict, sure, and formal demonstration.

Convinced in my own mind, that there can be neither paradoxes nor mysteries inherent and inexplicable in a system of characters of our own invention, and combined according to rules, the origin and extent of which we can precisely ascertain, I have endeavoured, in the present memoir, to shew why certain conclusions obtained through the means of imaginary quantities are necessarily true: to effect this is my prime object; a subordinate one is, to shew that the method founded on imaginary symbols is commodious, and proper to be adopted, because of easy and extensive application.

It has been already observed, that demonstration ultimately depends on observations made on individual objects, and that a conclusion expressed by certain characters and signs, if general, must be true in each particular case that presents itself, on assigning specific values to the signs. After affixing a signification to the symbols $\times$, $+$, &c. the product of $(a + b)$ and $(c + d)$ can be proved equal to $(ac) + (ad) + (bc) + (bd)$; if $na = b$, a can be proved equal $\frac{b}{n}$, a, b, c, &c. being the signs of real quantities; but nothing can be affirmed concerning the product of $(a + b\sqrt{-1})$, and $(c + d\sqrt{-1})$, nor concerning the form $na = b\sqrt{-1}$; and all that can be meant by the form $(a + b\sqrt{-1}) \times (c + d\sqrt{-1})$ is, that the characters are to be combined after the same manner that the signs of real quantities are; so that $(a + b\sqrt{-1}) \times (c + d\sqrt{-1})$, and $ac + ad\sqrt{-1} + cb\sqrt{-1} - bd$, are two forms equivalent to each other, not proved equivalent, but put so, by extending the rule demonstrated for the signs of real quantities to characters that are insignificant.

In like manner $(a+b)^{x\sqrt{-1}}$ can never be proved equal to $a^{x\sqrt{-1}} + x\sqrt{-1}\,a^{x\sqrt{-1}-1}\,b + \&c.$ it is only an abridged symbol for the series; there can be no ambiguity in the meaning of $(a+b)^{x\sqrt{-1}}$, since it is intended to represent the series which arises from developing $(a+b)^{x\sqrt{-1}}$, after the same manner that $(a+b)^x$ is developed.

The symbol $\sqrt{-1}$ might arise from translating questions of which the statement involved a contradiction of ideas into algebraic language, and reasoning on them, as if they really admitted a solution. For instance, if it were required to divide the number 12 into two such parts, that their product should equal 37, this question in algebraic language would be $12x - x^2 = 37$; an absurd statement, since no real number can be assigned to x that verifies it; but, according to the rules for transposition, the equation $12x - x^2 = 37$, is equivalent to $x^2 - 12x + 36 = -1$. If x were the sign of a real quantity, $x - 6$, or $6 - x$, would be the square root of $x^2 - 12x + 36$; if therefore $\pm(x-6)$ be put for the square root, it is put so by extending the rule proved for real quantities to this case; and the radical placed over the symbol -1, shews that such extension has been assumed; hence $x - 6 = +\sqrt{-1}$ is an expression of which the origin is known, being derived from $x^2 - 12x + 36 = -1$.

In the present inquiry, it is immaterial how the symbol $\sqrt{-1}$ originated: I think its origin most probably accounted for thus. The determination of general rules for the combination of algebraic quantities, was probably posterior to the actual solution of many problems, effected by particular artifices. During the solutions, certain similar parcels of characters presented themselves,

which it was necessary either to combine or separate; and, to obtain general rules for their combination and separation, the first algebraists feigned forms similar to what really presented themselves in specific cases:* thus, in questions producing

* It has been already observed, that the determination of general rules for algebraic operations was posterior to the actual solutions of problems. To obtain a rule for the multiplication of algebraic quantities, a form such as $a - b + c - m$, was proposed to be multiplied by $d - e + f - n$; since it was necessary to have a law for the multiplication of the signs $+ -$, a general one was established, that like signs multiplied produce $+$, unlike $-$, either from proving such law when $(a + c)$ was $> b + m$, and $(d + f) > (e + n)$, or from remarking that, in the solution of problems, the observance of such law always produced true conclusions. It is very certain that the mind can form no idea of an abstract negative quantity; and therefore nothing can be affirmed concerning the multiplication of $-a$, and $-b$, nor of $a - b$, and $c - d$, if a is $< b$, or $c < d$. Let us attend, however, to the real meaning of negative quantities, and to the cause of their appearance in the solution of problems. The rule for transposition is, that quantities may be transferred from one side of the equation to the other, changing their signs. By virtue of this rule, an equation may appear under the form $-x = a - b$, $(a < b)$, or $x - y = a - b$, $x < y$, $a < b)$; which equations, abstractedly considered, may appear absurd, but become intelligible by means of the equations $x = b - a$, $y - x = b - a$, to which they are significant, and to which they may be immediately reduced. Suppose now, $-x = a - b$ is to be multiplied by $-z = m - n$ $(m < n)$; if the forms be reduced to their equivalent ones, $x = b - a$, and $z = n - m$, and then multiplied, the product may be proved $xz = bn - bm - an + am$. Now, let $(a - b)$ be multiplied by $(m - n)$, in the same manner as it ought to be if a were $> b$, $m > n$, and the product is, $(am - an - bm + bn)$, or $(bn - bm - an + am)$, the same as arose from multiplying $b - a$ by $n - m$, and which is equal xz; hence $-x \times -z$ must be put xz; hence, in multiplication, we are sure to have right results by always observing the law that the product of like signs is $+$, of unlike $-$. In a similar manner it may be proved, that the product of $x - y = a - b$, by $m - n = d - c$, will be truly expressed by combining the quantities according to the same law for the signs. It is evident how much the establishment of this law must facilitate calculation; since, without considering whether x is greater or less than y, the product of $(x - y)$ and of $(m - n)$ may immediately be put down. Such equations as $x - y = a - b$, $(< y)$ must frequently occur in calculation, unless every step of the process be rendered extremely

quadratic equations, forms such as $x^2 - 7x + 10$, $x^2 + 3x - 10$, appeared; and therefore, to obtain a general rule for the solution of all like forms, $x^2 \pm ax \pm b$ was invented; and the solution, being made general, was necessarily extended to those cases which admitted no real answer. When such an extension is assumed, it is always indicated by the symbol $\sqrt{-1}$; and hence, to know what operations are to be performed with the symbol $\sqrt{-1}$, it is necessary to recur to the quadratic forms from which it is arbitrarily derived.

I now proceed to shew how sines, cosines, &c. may be expressed by means of exponential expressions; and, for the sake of perspicuity, I avoid all fluxionary operations, and adhere to a purely algebraical calculus.

To find the form for the developement of e^x, let $y = e^x$, or $y = \overline{1 + \overline{e-1}}|^x = \overline{\overline{1 + \overline{e-1}}|^n}|^{\frac{x}{n}}$, n being any quantity which disappears of itself in the value of y.

Now $\overline{1 + \overline{e-1}}|^n = 1 + n(e-1) + \frac{n.(n-1)}{2}(e-1)^2 +$ &c. $=$ (arranging the terms according to the powers of n) $1 + An + Bn^2 + Cn^3 +$, &c.

$A = (e-1) - \frac{1}{2}(e-1)^2 + \frac{1}{3}(e-1)^3 -$ &c. the values of B, C, &c. it is unnecessary to investigate, since they disappear in the calculation.

tedious by considerations on the relative value of quantities, and unless the rule for transposition be clogged with needless limitations: an abstract negative quantity is indeed unintelligible; but $-x = -a$, or $x - y = a - b$ $(x < y)$, are perfectly intelligible by means of their equivalent equations, $x = a$, $y - x = b - a$, to which they can be immediately reduced. The tendency of the reform proposed to be introduced into algebra is, it appears to me, to destroy the chief advantages of that art; its compendious and expeditious methods of calculation.

Hence $y = (1 + An + Bn^2 + Cn^3 + \&c.)^{\frac{x}{n}}$

$$= 1 + \frac{x}{n}(An + Bn^2 + \&c.) + \frac{x.(x-n)}{2n^2}(An + Bn^2 + \&c.)^2$$

$$= 1 + x(A + Bn + \&c.) + \frac{x.(x-n)}{2}(A + Bn + \&c.)^2.$$

Now, since n is arbitrary, and ought, by the nature of the function y, to disappear from the expression of the function, it follows, that all terms multiplied by each power of n must destroy each other; neglecting, therefore, the terms which ought of themselves to disappear, whatever n is, we have simply,

$$y = e^x = 1 + Ax + \frac{A^2x^2}{1.2} + \frac{A^3x^3}{1.2.3} + \&c.$$

$$\text{if } A = 1) = 1 + x + \frac{x^2}{1.2} + \frac{x^3}{1.2.3} + \&c.^*$$

This demonstration for the developement of e^x is general, whatever x is, provided it is always the sign of a real quantity; but $e^{x\sqrt{-1}}$ can never be proved equal to $1 + x\sqrt{-1} - \frac{x^2}{1.2} - \frac{x^3\sqrt{-1}}{1.2.3} + \&c.$† What then is to be understood by $e^{x\sqrt{-1}}$? merely this, that $e^{x\sqrt{-1}}$ is an abridged symbol for the series of characters $1 + x\sqrt{-1} - \frac{x^2}{1.2} - \&c.$ not proved, but assumed, by extending the form really belonging to e^x to $e^{x\sqrt{-1}}$.

In like manner, $e^{-x\sqrt{-1}}$ is an abridged symbol for

* This demonstration is due to M. LAGRANGE.

† In all treatises, after the demonstration for the developement of e^x, $e^{x\sqrt{-1}}$ is put $1 = x\sqrt{-1} - \frac{x^2}{1.2} - \&c.$ as if this case was really included in the general one of e^x.

$1 - x\sqrt{-1} - \frac{x^2}{1 \cdot 2} -$, &c. $e^{(x+y)\sqrt{-1}}$ an abridged symbol for $1 + (x+y)\sqrt{-1} - \frac{(x+y^2)}{1 \cdot 2} -$ &c. and there can be no ambiguity in what these symbols are meant to represent; since we have only in the demonstrated form $1 + x + \frac{x^2}{1 \cdot 2} +$ &c. to substitute $x\sqrt{-1}$, $-x\sqrt{-1}$, or $(x+y)\sqrt{-1}$, for x.

The use made of these abridged symbols is, to express, in an algebraic form, certain lines belonging to the circle, as sines, cosines, &c. for, since

$e^{x\sqrt{-1}}$ is an abridged symbol for $1 + x\sqrt{-1} - \frac{x^2}{1 \cdot 2} - \frac{x^3\sqrt{-1}}{1 \cdot 2 \cdot 3}$, &c.

and $e^{-x\sqrt{-1}}$ for $1 - x\sqrt{-1} - \frac{x^2}{1 \cdot 2} + \frac{x^3\sqrt{-1}}{1 \cdot 2 \cdot 3}$, &c.

$\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}$ is a symbol for $1 - \frac{x^2}{1 \cdot 2} + \frac{x^4}{1 \cdot 2 \cdot 3 \cdot 4} -$ &c.

$\frac{e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}}{2\sqrt{-1}}$ is a symbol for $x - \frac{x^3}{1 \cdot 2 \cdot 3} + \frac{x^5}{1 \cdot 2 \cdot 3 \cdot 4 \cdot 5} -$ &c.

but $1 - \frac{x^2}{1 \cdot 2} +$ &c. and $x - \frac{x^3}{1 \cdot 2 \cdot 3} +$ &c. represent the cosine and sine of an arc x $\therefore$ $\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}$, and * $\frac{e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}}{2\sqrt{-1}}$, in consequence of the assumptions made, properly represent the sine and cosine of an arc x.

* The usual method of deducing these expressions, is by a fluxionary process. I have preferred an algebraical one, for the sake of perspicuity. In an algebraical investigation, every step may be closely examined, and we can easily retrace to the original notions from which it commenced. In fluxions, the significancy of the expressions, and the nature and manner of their derivation, demand much time and attention, to be properly understood. If, however, the fluxionary process be examined, its object will appear to be, to find out a method of abridgedly representing the sine, &c. of an arc, employing for that purpose a form demonstrated for real quantities. In this fluxionary process, it is quite unnecessary to mention either the hyperbola or logarithms.

To remove all doubt and occasion of cavil, it is to be understood, that $(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}})$ means, that the terms of the series which $e^{x\sqrt{-1}}$ represents, are to be connected with the terms of the series that $e^{-x\sqrt{-1}}$ represents, according to the rules obtaining for the addition of real quantities: again, that $x\sqrt{-1} - x\sqrt{-1}$ is put equal o, not by bringing $x\sqrt{-1}$ under the predicament of quantity, and making it the subject of arithmetical computation, but by giving to $+$ and $-$ their proper signification when used with real quantities, and then they designate reverse operations: again, that $\frac{x\sqrt{-1}}{\sqrt{-1}}$ is equal to x, not because it is true that a quantity multiplied and divided by the same number remains the same, but because $\frac{x\sqrt{-1}}{\sqrt{-1}}$ means, that x is to be combined with $\sqrt{-1}$ after the manner that real quantities are in multiplication, and then divided after the manner that real quantities are in division; and therefore, since the two operations are the reverse of each other, $\frac{x \times \sqrt{-1}}{\sqrt{-1}}$ and x must be equivalent expressions.*

To facilitate the solution of the propositions demonstrated by means of imaginary quantities, I previously observe, that, A being any symbol whatever, $A \times (e^{x\sqrt{-1}} + e^{-x\sqrt{-1}} - e^{y\sqrt{-1}})$, and $A\,e^{x\sqrt{-1}} + A\,e^{-x\sqrt{-1}} - A\,e^{y\sqrt{-1}}$, are equivalent ex-

* After this manner ought to be interpreted what MACLAURIN and BERNOUILLI have indistinctly expressed, concerning the compensation that ought to take place when real quantities are represented by means of imaginary symbols.—BERN. Vol. I. No. 70. MACL. Fluxions, Art. 699, 763.

pressions; for the same series results, whether the terms of the developement for $e^{x\sqrt{-1}}$, $e^{-x\sqrt{-1}}$, $e^{y\sqrt{-1}}$ be connected together after the manner pointed out by the signs $+ -$, and then combined with A, or whether A be first separately combined with each term of $e^{x\sqrt{-1}}$, $e^{-x\sqrt{-1}}$, $e^{y\sqrt{-1}}$, and then the resulting terms added together: again, $e^{x\sqrt{-1}} \times e^{y\sqrt{-1}}$, and $e^{(x+y)\sqrt{-1}}$, are equivalent expressions; for the same series results, whether the terms for the developements of $e^{x\sqrt{-1}}$ and $e^{y\sqrt{-1}}$ be connected together after the manner of quantities in multiplication, or whether $e^{(x+y)\sqrt{-1}}$ be immediately developed, by putting $(x+y)\sqrt{-1}$ for x, in the series $1 + x + \frac{x^2}{1.2} + \frac{x^3}{1.2.3} +$ &c.

for $e^{x\sqrt{-1}}$ is the symbol for $1 + x\sqrt{-1} - \frac{x^2}{1.2} - \frac{x^3\sqrt{-1}}{1.2.3} +$ &c.

$e^{y\sqrt{-1}}$ is the symbol for $1 + y\sqrt{-1} - \frac{y^2}{1.2} - \frac{y^3\sqrt{-1}}{1.2.3} +$ &c.

$\therefore$ $e^{x\sqrt{-1}} \times e^{y\sqrt{-1}}$ (the symbol $\times$ indicating that the several terms are to be connected together according to the rules of multiplication) equals $1 + (x+y)\sqrt{-1} - \left(\frac{(x+y)^2}{2}\right) -$ &c. which series is abridgedly expressed by the symbol $e^{(x+y)\sqrt{-1}}$.

$\therefore$ $e^{x\sqrt{-1}} \times e^{y\sqrt{-1}}$, and $e^{(x+y)\sqrt{-1}}$, are symbols alike significant; or, since it must now be evident in what sense the equality of imaginary expressions is to be understood, $e^{x\sqrt{-1}} \times e^{y\sqrt{-1}} = e^{(x+y)\sqrt{-1}}$.

After this explanation of the nature of the operations directed by means of certain signs $\times, +$, &c. to be performed with the symbols $e^{x\sqrt{-1}}$, $e^{y\sqrt{-1}}$, &c. the following propositions may be clearly and strictly proved.

1. Sin. x . cos. $y = \frac{1}{2}$ sin. $(x + y) + \frac{1}{2}$ sin. $(x - y)$.

For sin. $x = \frac{e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}}{2\sqrt{-1}}$, cos. $y = \frac{e^{y\sqrt{-1}} + e^{-y\sqrt{-1}}}{2}$;

$\therefore$ sin. $x \times$ cos. $y = \left(\frac{e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}}{2\sqrt{-1}}\right) \times \left(\frac{e^{y\sqrt{-1}} + e^{-y\sqrt{-1}}}{2}\right)$

= by what has been shewn,

$$\tfrac{1}{2} \times \frac{e^{x\sqrt{-1}}}{2\sqrt{-1}} \times \left(e^{y\sqrt{-1}} + e^{-y\sqrt{-1}}\right) - \tfrac{1}{2} \times \frac{e^{-x\sqrt{-1}}}{2\sqrt{-1}} \times \left(e^{y\sqrt{-1}} + e^{-y\sqrt{-1}}\right)$$

$$= \tfrac{1}{2} \times \frac{e^{(x+y)\sqrt{-1}} + e^{(x-y)\sqrt{-1}}}{2\sqrt{-1}} - \tfrac{1}{2} \times \frac{e^{-(x-y)\sqrt{-1}} + e^{-(x+y)\sqrt{-1}}}{2\sqrt{-1}}$$

$$= \tfrac{1}{2} \times \frac{e^{(x+y)\sqrt{-1}} - e^{-(x+y)\sqrt{-1}}}{2\sqrt{-1}} - \tfrac{1}{2} \times \frac{e^{(x-y)\sqrt{-1}} - e^{-(x+y)\sqrt{-1}}}{2\sqrt{-1}}$$

$= \frac{1}{2} \times$ sin. $(x + y) + \frac{1}{2}$ sin. $(x - y)$.

2. $\overline{\text{cos. } x}^{\,n} = \frac{1}{2^{n-1}} \{$cos. $n x + n$. cos. $(n-2)x + \frac{n.(n-1)}{2}$ cos. $(n-4)x$ + &c.

$+ \frac{1}{2^{n-1}} \times \frac{(n+3)(n+5)(n+7)\ldots\ldots 2n}{(n-1)(n-3)(n-5)\ldots\ldots 5 . 3}$ cos. (x) (n an odd number) or

$= \frac{1}{2^{n-1}} \{$cos. $n x + n$. cos. $(n-2)x + \frac{n.(n-1)}{2}$ cos. $(n-4)x$ + &c.

$+ \frac{(n+2)(n+4)\ldots . 2n}{n.(n-2)\ \ldots 4 . 2} \times \frac{1}{2}$, n being an even number:

for cos. $x = \frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}$ $\therefore \overline{\text{cos. } x}^{\,n} = \left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)^n$

$= \left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)^{n-1} \times \left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)$; now,

if $\left(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}\right)^{n-1}$ were $= e^{(n-1)x\sqrt{-1}} + (n-1)$ $e^{(n-3)x\sqrt{-1}}$ + &c. $\left(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}\right)^n$ would $= e^{nx\sqrt{-1}}$ $+ n\, e^{(n-2)x\sqrt{-1}}$ + &c. or, if the developement of $\left(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}\right)^{n-1}$ were according to the law of the binomial theorem, the developement of $\left(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}\right)^n$ would be according to the same law; but the developement of

$\left(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}\right)^2$ is according to that law $\therefore$ of $\left(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}\right)^3 \therefore$ &c. $\therefore \left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)^n$

$$= \frac{1}{2^n}\left(e^{nx\sqrt{-1}} + n e^{(n-2)x\sqrt{-1}} + \frac{n(n-1)}{2} e^{(n-4)x\sqrt{-1}} + \text{\&c.}\right.$$

$$+ \frac{(n+3)(n+5)\ldots 2n}{(n-1)(n-3)\,n-5\cdot\cdot 3} \times \left(e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}\right) + \text{\&c.}$$

$$\left. + n e^{-(n-2)x\sqrt{-1}} + e^{-nx\sqrt{-1}}\right) (n \text{ odd}), \text{ or}$$

$$= \frac{1}{2^{n-1}}\left(\frac{e^{nx\sqrt{-1}} + e^{-nx\sqrt{-1}}}{2}\right) + n \times \left(\frac{e^{(n-2)x\sqrt{-1}} + e^{-(n-2)x\sqrt{-1}}}{2}\right) + \text{\&c.}$$

$$+ \frac{(n+3)(n+5)\ldots 2n}{(n-1)(n-3)\,n-5\cdot\cdot 3} \times \left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)$$

$$= \frac{1}{2^{n-1}}\left(\cos. nx + n \cdot \cos.(n-2)x + \text{\&c.} + \frac{(n+3)(n+5)\ldots 2n}{(n-1)(n-3)\ldots 3} \cos. x\right)$$

when n is even

$$\left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)^n = \frac{1}{2^n} \times \left(e^{nx\sqrt{-1}} + n e^{(n-2)x\sqrt{-1}}\right.$$

$$\left. + \frac{n\cdot(n-1)}{2} e^{(n-4)x\sqrt{-1}} + \text{\&c.} + \frac{(n+2)(n+4)\ldots 2n}{n\cdot(n-2)(n-4)\cdot\cdot 4\cdot 2}\right)$$

$$= \frac{1}{2^{n-1}} \times \left(\frac{e^{nx\sqrt{-1}} + e^{-nx\sqrt{-1}}}{2}\right) + n \times \left(\frac{e^{(n-2)x\sqrt{-1}} + e^{-(n-2)x\sqrt{-1}}}{2}\right) + \text{\&c.}$$

$$+ \frac{(n+4)(n+6)\ldots 2n}{(n-2)(n-4)\ldots 2} \times \left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right) + \frac{(n+2)(n+4)\ldots 2n}{n\cdot(n-2)-1\cdot\cdot 2} \times \frac{1}{2}$$

$$= \frac{1}{2^{n-1}} \times \left(\cos. nx + n \cdot \cos.(n-2)x + \text{\&c.} \frac{(n+4)(n+6)\ldots 2n}{(n-2)(n-4)\ldots 2} \cos. 2x\right.$$

$$\left. + {}^{*}\frac{(n+2)(n+4)\ldots 2n}{n\cdot(n-2)\ldots 2} \times \frac{1}{2}\right).$$ By a similar investigation, $\overline{\text{sine } x}^n$ may be determined.

* This is the greatest coefficient in the binomial, and belongs to the term in which the index of e is 0; for the coefficient of the mth term is $\frac{n(n-1)(n-2)\ldots(n-m+2)}{1\cdot 2\cdot 3 \ldots m-1}$,

3. The sum of $\cos. x + \cos. 2x + \cos. 3x \ldots. + \cos. nx$ $= \frac{\cos. nx - \cos. (n+1)x + \cos. x - 1}{2(1-\cos. x)}$, for this sum is

$$\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2} + \frac{e^{2x\sqrt{-1}} + e^{-2x\sqrt{-1}}}{2} + \&c. \ldots \frac{e^{nx\sqrt{-1}} + e^{-nx\sqrt{-1}}}{2},$$

or $\quad \frac{1}{2} \times \left(e^{x\sqrt{-1}} + e^{2x\sqrt{-1}} + \ldots\ldots\ldots + e^{nx\sqrt{-1}}\right)$
$+ \frac{1}{2} \times \left(e^{-x\sqrt{-1}} + e^{-2x\sqrt{-1}} + \&c. \ldots\ldots\ldots e^{-nx\sqrt{-1}}\right)$.

Now, according to the explanation that has been given of equality,

$$e^{x\sqrt{-1}} + e^{2x\sqrt{-1}} \ldots e^{nx\sqrt{-1}} = e^{x\sqrt{-1}} \times \left(1 + e^{x\sqrt{-1}} + e^{2x\sqrt{-1}} \ldots e^{\overline{n-1}\,x\sqrt{-1}}\right)$$

if S were an abridged symbol, which, developed according to a certain form, became the series $e^{x\sqrt{-1}} + e^{2x\sqrt{-1}} \ldots e^{nx\sqrt{-1}}$, then $1 + S - e^{nx\sqrt{-1}}$ would truly represent $1 + e^{x\sqrt{-1}} + e^{2x\sqrt{-1}} \ldots e^{(n-1)x\sqrt{-1}}$, and therefore S and $e^{x\sqrt{-1}} \times \left(1 + S - e^{nx\sqrt{-1}}\right)$ would be expressions equally significant, or S would $= e^{x\sqrt{-1}} + Se^{x\sqrt{-1}} - e^{\overline{(n+1)}x\sqrt{-1}}$, or $S = \frac{e^{(n+1)x\sqrt{-1}} - e^{x\sqrt{-1}}}{e^{x\sqrt{-1}} - 1}$, the form that must be given to S: so that, when expanded after a known form, it becomes $e^{x\sqrt{-1}} + e^{2x\sqrt{-1}} \ldots\ldots e^{nx\sqrt{-1}}$.

In like manner, the abridged symbol for $e^{-x\sqrt{-1}} + e^{-2x\sqrt{-1}} \ldots e^{-nx\sqrt{-1}}$

but index $n - 2m + 2 = 0 \therefore 2m = n + 2 \therefore$ the coefficient is $\frac{n.(n-1)\ldots\left(\frac{n+4}{2}\right)\frac{(n+2)}{2}}{1.2.3\ldots\frac{(n-2)}{2}\frac{n}{2}}$ $= \frac{(n+2)(n+4)\ldots\ldots(2n-2)2n}{n.(n-2)(n-4)\ldots\ldots4.2}$; and it is the greatest coefficient, since the coefficients of the adjacent terms are determined by multiplying it by $\frac{m-1}{n-m+2}$ and $\frac{n-m+1}{m}$ respectively; or, since $m = \frac{n+2}{2}$, by $\frac{n}{n+2}$ and $\frac{n}{n+2}$.

is $\frac{e^{-(n+1)x\sqrt{-1}} - e^{-x\sqrt{-1}}}{e^{-x\sqrt{-1}} - 1}$. Hence the series is represented

by $\frac{1}{2}\left(\frac{e^{(n+1)x\sqrt{-1}} - e^{x\sqrt{-1}}}{e^{x\sqrt{-1}} - 1}\right) + \frac{1}{2}\frac{e^{-(n+1)x\sqrt{-1}} - e^{-x\sqrt{-1}}}{e^{-x\sqrt{-1}} - 1}$;

which is the same as

$$\frac{1}{2} \times \frac{e^{(n+1)x\sqrt{-1}} - e^{x\sqrt{-1}}}{e^{x\sqrt{-1}} - 1} \times \frac{e^{-x\sqrt{-1}} - 1}{e^{-x\sqrt{-1}} - 1} + \frac{1}{2}\frac{e^{-(n+1)x\sqrt{-1}} - e^{-x\sqrt{-1}}}{e^{-x\sqrt{-1}} - 1}$$

$$\times \frac{e^{x\sqrt{-1}} - 1}{e^{x\sqrt{-1}} - 1}, \text{ or is}$$

$$= \frac{1}{2} \times \frac{e^{nx\sqrt{-1}} - 1 - e^{(n+1)x\sqrt{-1}} + e^{x\sqrt{-1}}}{2 - e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}} \; \ldots\ldots$$

$$+ \frac{1}{2} \times \frac{e^{-nx\sqrt{-1}} - 1 - e^{-(n+1)x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2 - e^{x\sqrt{-1}} + e^{x\sqrt{-1}}}$$

$$= \frac{\frac{1}{2}\left(\frac{e^{nx\sqrt{-1}} + e^{-nx\sqrt{-1}}}{2}\right) - \left(\frac{e^{x(n+1)\sqrt{-1}} + e^{-(n+1)x\sqrt{-1}}}{2}\right) + \left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right) - 1}{2 \times \left(1 - \frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)}$$

$$= \frac{\cos. nx - \cos. (n+1)x + \cos. x - 1}{2 \times (1 - \cos. x)}.$$

In like manner, the series $\sin. x + \sin. 2x + \ldots \sin. mx$ may be shewn $= \frac{\sin. x - \sin. (m+1)x + \sin. mx}{2(1 - \cos. x)}$. The sum of $\overline{\cos. x}^2 + \overline{\cos. 2x}^2 \ldots \overline{\cos. nx}^2$ may easily be found, by expressing it under the form $\left(\frac{e^{x\sqrt{-1}} + e^{-x\sqrt{-1}}}{2}\right)^2 + \left(\frac{e^{2x\sqrt{-1}} + e^{-2x\sqrt{-1}}}{2}\right)^2 + \ldots \left(\frac{e^{nx\sqrt{-1}} + e^{-nx\sqrt{-1}}}{2}\right)^2$; which, by expanding the terms,

becomes $\left(\frac{e^{2x\sqrt{-1}} + 2 + e^{-2x\sqrt{-1}}}{4}\right) + \left(\frac{e^{4x\sqrt{-1}} + 2 + e^{-4x\sqrt{-1}}}{4}\right) +$ &c.

and, consequently, equals $\frac{n}{2} + \frac{1}{2}\cos.(2x) + \cos.(4x) + \cos.(6x) \ldots \cos.(2nx)$, or $\frac{n}{2} + \frac{1}{2}\,\frac{\cos.2nx - \cos.(2n+2)x + \cos.2x - 1}{2(1-\cos.2x)}$.

Similarly may be determined the sums of $\overline{\sin.x}^2 + \overline{\sin.2x}^2 + \ldots \overline{\sin.nx}^2$, of $\overline{\cos.x}^3 + \overline{\cos.2x}^3 + \ldots \overline{\cos.nx}^3$, and generally of $\overline{\cos.x}^n + \overline{\cos.2x}^n + \ldots\ldots \overline{\cos.mx}^n$; for, ($n$ being even,* $A = \frac{(n+2)(n+4)(n+6)\ldots 2n}{n\,(n-2)(n-4)\ldots 4.2}$, and $B = \frac{(n+4)(n+6)\ldots 2n}{(n-2)(n-4)\ldots 4.2}$)

$$\overline{\cos.x}^n = \frac{1}{2^{n-1}}\left(\cos.nx + n.\cos.(n-2)x + \text{\&c.} \ldots B\cos.2x + \frac{A}{2}\right)$$

$$\overline{\cos.2x}^n = \frac{1}{2^{n-1}}\left(\cos.2nx + n.\cos.2(n-2)x + \text{\&c.} \ldots B\cos.4x + \frac{A}{2}\right) \text{\&c.}$$

$$\overline{\cos.mx}^n = \frac{1}{2^{n-1}}\left(\cos.mnx + n.\cos.m(n-2)x + \text{\&c.} \ldots B.\cos.2mx. + \frac{A}{2}\right)$$

* The coefficient $A = \frac{(n+2)(n+4)(n+6)\ldots\ldots 2n}{n.(n-2)(n-4)\ldots 4.2}$ may be differently expressed, thus,

$$A = \frac{n.(n-1)(n-2)\ldots\ldots\ldots n-m+2}{(m-1)(m-2)(m-3)\ldots\ldots 4.3.2.1}\ (n-2m+2=0)$$

$$= \frac{\left(\frac{n}{2}+\frac{n}{2}\right).(n-1).\left(\frac{n}{2}-1+\frac{n}{2}-1\right).(n-3).\left(\frac{n}{2}-2+\frac{n}{2}-2\right)\ldots\left(\frac{n}{2}+1\right)}{\frac{n}{2}.\left(\frac{n}{2}-1\right)\left(\frac{n}{2}-2\right)\ldots\ldots\ldots\ldots \frac{n}{4}\left(\frac{n}{4}-1\right)\left(\frac{n}{4}-2\right)\ldots\ldots 4.3.2.1}$$

$$= \frac{2(n-1)\,2(n-3)\,2(n-5)\ldots\ldots\ldots 2\left(\frac{n}{2}+1\right)}{\frac{n}{4}.\left(\frac{n}{4}-1\right)\left(\frac{n}{4}-2\right)\ldots\ldots\ldots 4.3.2.1}$$

$$= \frac{2^{\frac{n}{4}}(n-1)(n-3)(n-5)\ldots\left(\frac{n}{2}+1\right)}{\frac{n}{4}\left(\frac{n}{4}-1\right)\left(\frac{n}{4}-2\right)\ldots 4.3.2.1} = \frac{2^{\frac{n}{2}}(n-1)(n-3)\ldots\left(\frac{n}{2}+1\right)}{\frac{n}{2}\left(\frac{n}{2}-2\right)\left(\frac{n}{2}-4\right)\ldots 8.6.4.2}$$

$$= \frac{2^{\frac{n}{2}}(n-1)(n-3)(n-5)\ldots\frac{n}{2}+1}{\frac{n}{2}\left(\frac{n}{2}-2\right)\left(\frac{n}{2}-4\right)\ldots 6.4.2} \times \frac{\left(\frac{n}{2}-1\right)\left(\frac{n}{2}-3\right)\left(\frac{n}{2}-5\right)\ldots 5.3.1}{\left(\frac{n}{2}-1\right)\left(\frac{n}{2}-3\right)\left(\frac{n}{2}-5\right)\ldots 5.3.1}$$

$$= \frac{2^{\frac{n}{2}}(n-1)(n-3)\ldots\ldots 5.3.1}{\frac{n}{2}\left(\frac{n}{2}-1\right)\ldots\ldots 6.5.4.3.2.1}, \text{ or } \frac{1.3.5\ldots(n-1)}{1.2.3\ldots\frac{n}{2}}\,2^{\frac{n}{2}}.$$

$\therefore$ adding the quantities that are vertical to each other, the series

$$= \frac{1}{2^{n-1}} \left(\cos. n x + \cos. 2 n x + \&c. \ldots \cos. m n x\right)$$
$$+ \frac{n}{2^{n-1}} \left(\cos. (n-2) x + \cos. 2 (n-2) x + \&c. \ldots \cos. m (n-2) x\right)$$
$$+ \&c.$$
$$+ \frac{B}{2^{n-1}} \left(\cos. 2 x + \cos. 4 x + \&c. \ldots \cos. 2 m x\right)$$
$$+ \frac{m A}{2^{n-1}} \left(\frac{1}{2}\right).$$

Now, each horizontal row consists of a series of cosines of arcs in arithmetical progression; and the sum of each series may immediately be obtained from the expression deduced in proposition 3d.

I think it superfluous to give more examples, since the object of this memoir is rather to shew the logical justness of a method, than its commodiousness or extent: all other propositions relative to lines drawn in a circle, when expressed by aid of the symbol $\sqrt{-1}$, the same principle of explanation regulates; the principle once understood, the operations become mechanical, require attention, but are attended with no real mental difficulty.

It is inaccurate to call $\frac{\varepsilon^{x\sqrt{-1}} + \varepsilon^{-x\sqrt{-1}}}{2}$ an imaginary value of the cosine of an arc: the expression expanded is a real one. By use of the symbol $\sqrt{-1}$, and of the forms proved to obtain in the combination of real quantities, a mode of notation is obtained, by which we may express sines and cosines, &c. relatively to their arc.

If the process by which the foregoing propositions have been established require illustration, I would ask what demonstration is, when the characters employed are signs of ideas, or repre-

sentatives of real things; and demonstration would be defined to be, a method of shewing the agreement of remote ideas by a train of intermediate ideas, each agreeing with that next it; or, in other words, a method of tracing the connection between certain principles and a conclusion, by a series of intermediate and identical propositions, each proposition being converted into its next, by changing the combination of signs that represent it, into another shewn to be equivalent to it.

Exactly according to this plan have the foregoing propositions been demonstrated: the symbol for the sine of x is $\frac{e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}}{2\sqrt{-1}}$, for the cosine of y is $\frac{e^{y\sqrt{-1}} + e^{-y\sqrt{-1}}}{2}$, and the connection was traced between $\left(\frac{e^{x\sqrt{-1}} - e^{-x\sqrt{-1}}}{2\sqrt{-1}}\right) \times \left(\frac{e^{y\sqrt{-1}} + e^{-y\sqrt{-1}}}{2}\right)$ and $\frac{1}{2} \times \frac{e^{(x+y)\sqrt{-1}} - e^{-(x+y)\sqrt{-1}}}{2\sqrt{-1}}\Big) + \frac{1}{2} \times \frac{e^{(x-y)\sqrt{-1}} - e^{-(x-y)\sqrt{-1}}}{2\sqrt{-1}}$, by a series of transformations, each of which was shewn to be lawful, by referring to what $e^{x\sqrt{-1}}$ &c. was made to represent, and to the nature of the operations directed to be performed by the signs $\times$, $+$, &c. thus, the transformation of $e^{x\sqrt{-1}} \times \left(e^{y\sqrt{-1}} + e^{-y\sqrt{-1}}\right)$ into $e^{x\sqrt{-1}} \times e^{y\sqrt{-1}} + e^{x\sqrt{-1}} \times e^{-y\sqrt{-1}}$ is lawful, because the same series results, whether $e^{y\sqrt{-1}}$ and $e^{-y\sqrt{-1}}$ be first expanded, and then each term of their sum be combined with $e^{x\sqrt{-1}}$, or whether $e^{x\sqrt{-1}}$ be separately combined with each term of the developements for $e^{y\sqrt{-1}}$ and $e^{-y\sqrt{-1}}$, and then the resulting terms added together: again, the transformation of $e^{x\sqrt{-1}} \times e^{y\sqrt{-1}}$ into $e^{(x+y)\sqrt{-1}}$ is lawful, because the same series results, whether

$e^{x\sqrt{-1}}$ and $e^{y\sqrt{-1}}$ be expanded, and then their terms combined according to the rules for the multiplication of quantities, or whether $e^{(x+y)\sqrt{-1}}$ be immediately expanded, by writing $(x+y)\sqrt{-1}$ for x, in the series for e^x.

The other demonstrations examined will appear conducted on the same principle, which is simple, and of easy and immediate application: hence, although the symbol $\sqrt{-1}$ be beyond the power of arithmetical computation, the operations in which it is introduced are intelligible, and deserve, if any operations do, the name of reasoning.

It is almost superfluous to observe, that if the operations by means of imaginary symbols have appeared to be necessarily true, the arguments founded on the analogy subsisting between the circle and hyperbola must be abandoned, as unsatisfactory. What has been proved concerning the properties of lines appertaining to a circle, has been so without any mention of the hyperbola; and I may say, without danger of refutation, that the demonstrations would be strictly true, if such a curve as the hyperbola had never been invented. Add to this, that imaginary expressions are useful in leading to just conclusions, in investigations purely algebraical.

The chief purpose of this Paper is fulfilled, if it has appeared that the operations with imaginary symbols possess the evidence and rigour of mathematical demonstration: whether it is convenient to use imaginary quantities in analytical investigation, must be determined on the grounds of abridgment and extensive application. In the cases that I have considered, imaginary expressions are not, I know, indispensably necessary: they are excluded from each of three different methods for the solution of propo-

sitions relative to lines belonging to a circle, given by M. LAGRAVE, by EULER, (Introductio in Analysin Infinitorum, p. 198.) and by BOSSUT. (Mem. de l'Acad. 1769, p. 453.) I am, however, of opinion, that the method of representing sines, cosines, &c. by their abridged algebraical symbols, (such as is given in this Paper,) is the most easy and extensive in its application.*

It will be consistent with the purpose of the present memoir, to consider some of the expressions which I imagine are alluded to, by those who complain of the abuses, paradoxes, &c. introduced by negative and impossible quantities.

The quantity $\frac{4 \log. \sqrt{-1}}{\sqrt{-1}}$, which JOHN BERNOUILLI proved to be the circumference of a circle, is merely an abridged symbol, founded on a form proved for real quantities: the sense in which it is to be understood is this, that if in the series for log. x, viz. $(x - x^{-1}) - \frac{1}{2}(x^2 - x^{-2}) + \frac{1}{3}(x^3 - x^{-3}) -$ &c. $\sqrt{-1}$ is substituted for x, and the terms multiplied by 4 and divided by $\sqrt{-1}$, the resulting series expresses the circumference of a circle.

The expressions

$$(1)\ \sin.(a + b\sqrt{-1}) = \tfrac{1}{2}(e^b + e^{-b}) \sin. a + \tfrac{\sqrt{-1}}{2}(e^b - e^{-b}) \cos. a,$$

$$(2)\ \cos.(a + b\sqrt{-1}) + \cos.(a - b\sqrt{-1}) = (e^b + e^{-b}) \cos. a,$$

are due to EULER: the sense in which alone they are to be understood is this, that the series which results from substituting

* M. BOSSUT does not sum any series beyond that of the fourth power of sines and cosines of arcs in arithmetical progression: he contents himself with saying, that the general law for $\overline{\cos. q}^n + \overline{\cos. 2q}^n$ &c. may easily be discovered.

$a+b\sqrt{-1}$ for x, in the series $x-\frac{x^3}{1.2.3}+\frac{x^5}{1.2.3.4.5}$ &c. proved for the sine of an arc x, is the same as what results from expanding e^b, e^{-b}, cos. a, sin. a, &c. and combining the terms after the manner directed by the signs $+$, $\times$, &c. A like explanation is to be given of the second expression.

If m be an integer, c the semi-circumference, and $a=\left(\frac{4m\pm 1}{2}\right)c$, then cos. $a=0$, and the first expression becomes sin. $(a+b\sqrt{-1})$ $=\frac{1}{2}(e^b+e^{-b})$ sin. a. According to the explanation I have given, this expression is very perspicuous and intelligible; but EULER, inattentive to its true meaning, gives it an air of mystery and paradox, when he says that an impossible arc may have a real sine.

The symbol $(\sqrt{-1})^{\sqrt{-1}}$, EULER proved equal to 0.20787957, &c. To understand its meaning, we must recur to the form from which it was derived: now, according to the definition that has been given of equality between imaginary expressions, it may be shewn that

$$(a+b\sqrt{-1})^{m+n\sqrt{-1}}=r^m e^{-nx}(\text{cos.}(mx+n\times l.r)+\sqrt{-1}\,\text{sin.}(mx+n\times l.r))$$

r being $=\sqrt{a^2+b^2}$, sin. $x=\frac{b}{r}$, cos. $x=\frac{a}{r}$.

Now, if a be put $=0$, $m=0$, $b=1$, $n=1$, the expression $(a+b\sqrt{-1})^{m+n\sqrt{-1}}$ becomes $(\sqrt{-1})^{\sqrt{-1}}$, and the expression to which it is equal becomes $e^{-\frac{c}{2}}$ (c circumference). Or the meaning of the symbol may be thus explained, x^x is the same as $e^{x\,\text{log.}\,x}$ $\therefore$ if $\sqrt{-1}$ be put for x, $(\sqrt{-1})^{\sqrt{-1}}=e^{\sqrt{-1}\,\text{log.}\sqrt{-1}}$; but it has appeared that log. $\sqrt{-1}$ is an abridged symbol for

$\frac{c}{4\sqrt{-1}}$; hence $(\sqrt{-1})^{\sqrt{-1}} = e^{-\frac{c}{4}}$, or $(\sqrt{-1})^{\sqrt{-1}}$, is an abridged symbol for the series $1 - \frac{c}{4} + \frac{c^2}{1 \cdot 2 \cdot 4^2} - \frac{c^3}{1 \cdot 2 \cdot 3 \cdot 4^3} + \&c.$

I do not pretend to say, that such expressions as the above are likely to occur in investigation, and to be practically useful; my sole concern is to shew, that they are perfectly intelligible, and the necessary consequences of certain assumptions.

The paradoxes and contradictions mutually alleged against each other, by mathematicians engaged in the controversy* concerning the application of logarithms to negative and impossible quantities, may be employed as arguments against the use of those quantities in investigation. The paradoxes and contradictions will quickly disappear, by adopting the same mode of explanation that has been already employed in this paper. The memoir of EULER is in some parts erroneous, and frequently unsatisfactory.

The use of a mathematical definition is, to deduce from it the properties of the thing defined; and, whatever definition of logarithms be taken, we either have immediately, or may deduce for the purpose of computation, an expression such as $y = e^x$, in which x is the logarithm of e^x to the base e; the developement of e^x has been proved to be $1 + \mathrm{A}x + \frac{\mathrm{A}^2 x^2}{1 \cdot 2} + \frac{\mathrm{A}^3 x^3}{1 \cdot 2 \cdot 3} + \&c.$ A being $= (e-1) - \frac{1}{2}(e-1)^2 + \frac{1}{3}(e-1)^3 - \&c.$

* This controversy exercised a long time the abilities of LEIBNITZ, BERNOUILLI, EULER, D'ALEMBERT, and FONCENEX. The Commercium Philosophicum et Mathematicum, published at Lausanne, in 1745, and containing the letters of the two first controvertists, I have never seen; but I presume, that all the essential arguments of the controversy are to be found in FONCENEX's Memoir, (Vol. I. Mem. de Turin,) in EULER's, (Mem. de Berlin, 1749.) and in D'ALEMBERT's Opuscules, Vol. I.

Now the question concerning the logarithms of negative quantities, in a precise form, and freed from its verbal ambiguities, is this; is the symbol which, substituted for x in the developement of e^x, makes y or $e^x = -1$, the sign of a real quantity or not?

In the expression e^x, x is the logarithm of e^x, and, by extension, $x\sqrt{-1}$ is to be called the logarithm of $e^{x\sqrt{-1}}$. Now $e^{x\sqrt{-1}}$ is the symbol for $1 + x\sqrt{-1} - \frac{x^2}{1.2} - \frac{x^3\sqrt{-1}}{1.2.3}$ &c.

or $\left(1 - \frac{x^2}{1.2} + \frac{x^4}{1.2.3.4} - \text{\&c.}\right) + \sqrt{-1}\left(x - \frac{x^3}{1.2.3} + \text{\&c.}\right)$

or $e^{x\sqrt{-1}}$ may be said to be $= \cos. x + \sqrt{-1} \sin. x$. Hence, $x\sqrt{-1}$ is the logarithm of $\cos. x + \sqrt{-1}\sin. x$; when the arc x is equal 0, or 2π, or 4π, or 6π, or generally $2m\pi$, its $\cos. x = 1$.

Hence 0 is log. 1,

or $2\pi\sqrt{-1}$ is log. 1,

or $4\pi\sqrt{-1}$, or generally $2m\pi\sqrt{-1}$, is log. 1.

Hence, if $y = 1$, the equation $y = e^x$ becomes $1 = e^{2m\pi\sqrt{-1}}$, (m being any number of the progression 0, 1, 2, 3, 4, &c.)

Again, if the arc x is equal π, or 3π, or 5π, or generally $(2m + 1)\pi$, its $\cos. x = -1$.

$\therefore$ of -1, either $\pi\sqrt{-1}$, or $3\pi\sqrt{-1}$, or $5\pi\sqrt{-1}$, or $(2m+1)\pi\sqrt{-1}$, is to be called the logarithm; hence, if $y = -1$, the equation $y = e^x$ becomes $-1 = e^{(2m+1)\pi\sqrt{-1}}$.

The meaning of the logarithms of 1 and -1 are then thus to be understood. If in the series

$1 + x + \frac{x^2}{1.2} + \frac{x^3}{1.2.3} + \frac{x^4}{1.2.3.4} +$ &c. for x be substituted

either $0, 2\pi\sqrt{-1}, 4\pi\sqrt{-1}, 6\pi\sqrt{-1} \ldots$ or $(2m\pi\sqrt{-1})$, the equation $1 = 1 + x + \frac{x^2}{1 \cdot 2}$ &c. becomes identical; and if, in the same equation, for x be substituted either $\pi\sqrt{-1}, 3\pi\sqrt{-1}, 5\pi\sqrt{-1}, \ldots$ or $(2m+1)\pi\sqrt{-1}$, the equation $-1 = 1 + x + \frac{x^2}{1 \cdot 2} +$ &c. becomes identical; for, substitute $2m\pi\sqrt{-1}$ for x in series for e^x, then it becomes

$$1 + (2m\pi\sqrt{-1}) - \frac{(2m\pi)^2}{1 \cdot 2} - \frac{(2m\pi)^3\sqrt{-1}}{1 \cdot 2 \cdot 3} + \frac{(2m\pi)^4}{1 \cdot 2 \cdot 3 \cdot 4},$$

or $1 - \frac{(2m\pi)^2}{1 \cdot 2} + \frac{(2m\pi)^4}{1 \cdot 2 \cdot 3 \cdot 4} -$ &c. $+ \sqrt{-1}\left(2m\pi - \frac{(2m\pi)^3}{1 \cdot 2 \cdot 3} + \text{\&c.}\right)$ or $\cos. 2m\pi + \sqrt{-1} \sin. 2m\pi$; but $\sin. 2m\pi = 0$ $\cos. 2m\pi = 1 \therefore 1 = 1$.

In like manner it will appear, that $-1 = -1$, if $(2m+1)\pi\sqrt{-1}$ be substituted for x.

Since $1 = e^{2m\pi\sqrt{-1}}$ $(1)^2 = e^{4m\pi\sqrt{-1}}$, and since $-1 = e^{(2m+1)\pi\sqrt{-1}}$ $(-1)^2 = e^{(2m+1)2\pi\sqrt{-1}}$; but, in the developement of $e^{(2m+1)2\pi\sqrt{-1}}$, the rule of $- \times - = +$ is observed $\therefore$ it must be observed on the other side of the equation; hence, if $(1)^2 = (-1)^2$, the only strict conclusion that can be drawn is this, that $e^{4m\pi\sqrt{-1}}$ and $e^{(2m+1)2\pi\sqrt{-1}}$ * developed, produce the same series. It is a false consequence that, since $(1)^2 = (-1)^2$ the logarithm of $(1)^2 =$ logarithm $(-1)^2$, the logarithms are the indices $4m\pi\sqrt{-1}$ and $(2m+1)2\pi\sqrt{-1}$.

* 1 is equal $e^{2\pi\sqrt{-1}}$ or $e^{4\pi\sqrt{-1}}$ &c. or $e^{2\pi\sqrt{-1}}$, and $e^{4\pi\sqrt{-1}}$ expanded are each $= 1$; no consequence can be drawn from what is true for quantities elevated to real powers, since $e^{\pi\sqrt{-1}}$ can only have the meaning assigned it, that of being an abridged symbol.

EULER, confounding the common meaning of logarithms with their scientific definition, granted that the log. $(1)^2$ was equal log. $(-1)^2$, and endeavoured to reconcile the contradictions that immediately followed from such a concession.

The arguments intended to prove that the logarithms of negative quantities were real, may easily be shewn to be nugatory. EULER, certainly too much attached to mere calculation, instead of directly opposing them, sought to divert their force. D'ALEMBERT asserted, that the two progressions 1, 2, 3, &c. -1, -2, -3, &c. might have the same series of logarithms, o, p, q, r, &c. This is true, if -2 means $2 \times (-1)$, -3, $3 \times (-1)$, &c. or the progression -1, -2, -3, &c. is the same as $1 \times (-1)$, $2 (-1)$, $3 (-1)$, &c. wherein (-1) is considered as an unit, or as (x) a sign of a real quantity. But the question is thus evaded; since -1, -2, -3, —, &c. is brought precisely under the same predicament as 1, 2, 3, 4, &c. The only real point of inquiry could be, whether, consistently with the system of logarithms established for positive quantities, the logarithms of negative quantities were real.

A second argument brought by BERNOUILLI and D'ALEMBERT was, that since $a : -a :: -a : a \therefore (a)^2 = (-a)^2 \therefore$ log. $(a)^2$ = log. $(-a)^2 \therefore 2$ log. $(+a) = 2$ log. $(-a) \therefore$ log $(a) =$ log (a). This proposition, affirmed by D'ALEMBERT to be strictly true, viz. $(a : -a :: -a : a)$, was granted to be so by EULER, although it ought to have been denied; since, thus abstractedly proposed, it is absurd and unintelligible, and impossible to be proved. If, however,* $(-a)^2$ be assumed $= (a)^2$, $(-1)^2 =$

* I have explained, in a former note, for what reasons, and in what circumstances, $-a \times -a$ is the same as $a \times a$.

$(1)^2$, then the equality log. $(-a)^2 =$ log. $(a)^2$ becomes intelligible; since it means that the measure of the ratio between $(-a)^2$ and $(-1)^2$ is equal the measure of the ratio between $(a)^2$ and $(-1)^2$; but then this argument becomes the same as the former, and is equally illusory; for $-a$ and -1 are in fact made a and 1. If logarithms be defined, the measures of ratios existing between real quantities, then it is absurd to attempt deducing the logarithms of negative quantities from any reasoning on the relation that 1 has to -1; since there is no necessary connection between 1 and -1; and, independently of certain assumptions, the ratio of $1 : -1$ is perfectly unintelligible. Indeed the question admits no other meaning than that I originally assigned it: if a form demonstrated for positive quantities be extended, then certain symbols may be exhibited, which, agreeably to such extension, are called the logarithms of negative quantities.

Other arguments than those I have mentioned, were drawn from the theories of curves and fluxions, not only foreign to the question, which was purely algebraical, but of small weight; had they been of greater, the inquiry would necessarily have been diverted on the nature of the connection existing between these theories and algebra.

In this controversy, the predominancy of the "Esprit Geometrique" is remarkable; if, in an inquiry purely mathematical, any ambiguity or paradox presents itself, the most simple and natural method is, to recur to the original notions on which calculation has been founded. Instead of pursuing this method, the controvertists sought to derive illustration from obscure doctrines, or to discover the latent truth amidst the complex forms and involutions of analysis.

My inquiry concerning impossible quantities, has been confined to their use in representing lines belonging to the circle, and to the necessary truth of the conclusions obtained by their means; led by the connection of the subjects, I have made a small deviation, to examine the true meaning of certain symbols, and the contradictions said to embarrass the doctrines of logarithms when applied to negative quantities. The use, however, of impossible quantities has been extended to all parts of analysis. By their aid are determined, the values of formulas that occur in the science of the motion of fluids, the numerators of partial fractions as $\left(\frac{Ax + B}{x^2 + 2\alpha x + \alpha^2 + \beta^2}\right)$, the developement of forms as $(r^2 - 2rr' \cos. z + r'^2)^{-m}$, and the integration of many differential equations.* If, in these cases, the operation

* By means of impossible quantities, CARDAN's rule, in the irreducible case, when the three roots are real and incommensurable, may be applied. In the equation $x^3 - px + r$, when $\frac{r^2}{4}$ is $< \frac{p^3}{27}$, the root appears under the form $\sqrt[3]{a + b\sqrt{-1}} + \sqrt[3]{a \mp b\sqrt{-1}}$; and, by putting $a + b\sqrt{-1} = r\,(\cos. x' + \sin. x' . \sqrt{-1})$, the three roots may easily be shewn to be $2\sqrt{\frac{p}{3}} \cos. \frac{x'}{3}$, $2\sqrt{\frac{p}{3}} \cos. \left(\frac{x' + 2c}{3}\right)$, $2\sqrt{\frac{p}{3}} \cos. \left(\frac{4c + x'}{3}\right)$.

This method, indeed, only exhibits the linear value of the root; the algebraic value cannot generally be exhibited. In some particular cases, the algebraic value may be obtained, when the series that results from adding the terms of the developements of $\sqrt[3]{a + b\sqrt{-1}}$ and $\sqrt[3]{a - b\sqrt{-1}}$ can be summed; as M. NICOLE (Mem. de l'Acad. 1738, pages 97, 244.) has shewn, who first proved the expression $\sqrt[3]{a + b\sqrt{-1}} + \sqrt[3]{a - b\sqrt{-1}}$, when expanded, to be real.

I am of opinion, that CARDAN's solution, in the irreducible case, cannot be extended so as to obtain the general linear value, or in particular cases the algebraic value, except by operations with impossible quantities; and that when, by aid of impossible quantities, the general linear value or particular algebraic values are exhibited, such

with imaginary symbols are intelligible and just, the only argument for their exclusion must be founded on the existence of methods more general and expeditious.

The application of imaginary quantities to the theory of equations, has perhaps been made more extensively than to any other part of analysis. To consider the propriety of this application on the grounds of perspicuity and conciseness, a long discussion would be necessary. I may, however, be here permitted merely to state my opinion, that impossible quantities must be employed in the theory of equations, in order to obtain general rules and compendious methods. The demonstration of the principal proposition, that every root of an equation is comprised under the form $M + N\sqrt{-1}$, and that consequently every equation of $2n$ dimensions, is always divisible into n quadratic factors, appears to me, I confess, deficient in evidence and mathematical rigour. To establish this proposition, and to prove likewise, that every imaginary expression derived from transcendental operations is always comprised under the form $M + N\sqrt{-1}$, is the object of two Memoirs by D'ALEMBERT and EULER. (Mem. de Berlin, 1746, 1749.)

M. FONCENEX, (Mem. de Turin.) LAGRANGE, (Mem. de Berlin, 1771, 1772, 1773,) LAPLACE, WARING, and other mathematicians, have directed their inquiries towards the same subject.*

values are necessarily and by strict consequence true; and not true because they may be verified by a distinct or more rigorous investigation, nor because the operations have a tacit and implied reference to other more legitimate operations.

* None of the demonstrations go farther than to shew the *possibility* of resolving an equation of $2n$ dimensions into n quadratic factors. The actual resolution of equations that pass the fourth degree, has not hitherto been executed. Of the labours of such learned men as those I have mentioned, I speak with the greatest diffidence: the mere knowledge, however, of the possibility of the resolution of equations, appears to

The nature of the subject has obliged me to give this paper, in several of its parts, somewhat of a controversial cast: for having used the freedom of discussion in matters of pure science, an apology is unnecessary; the memoir of the ingenious person whose opinion I have formally controverted, I can most sincerely commend for every thing, except the justness of the principle of explanation.

To excuse the prolixity that may appear in the explanation of the operations, and in the proofs of their justness, I wish it to be considered, that it was necessary to examine the notions on which calculation ultimately rests; to explain the meaning of imaginary symbols, by tracing their derivation; to establish by separate and independent proofs, rules for the combination of impossible quantities, and not by inference from their similarity to rules for like combinations of real quantities; and carefully to distinguish between what is proved on evident principles, and what is only consequent from arbitrary assumptions.

Mathematical science has been at times embarrassed with contradictions and paradoxes; yet they are not to be imputed to imaginary symbols, rather than to any other symbols invented for the purpose of rendering demonstration compendious and expeditious. It may, however, be justly remarked, that

me unimportant. A useful consequence from this possibility of resolution, is said to be the integration of the form $\left(\frac{P}{Q}\dot{x}\right)$, P and Q being rational functions of x; now, when $\frac{P}{Q}$ is expressed by a series of fractions, a form as $\frac{(Ax+B)\dot{x}}{x^2+2\alpha x+\alpha^2+\beta^2}$ presents itself to be integrated; but the actual value of the integration cannot be assigned, without knowing what A, B are; and A, B, cannot be determined, except α, β, are known.

mathematicians, neglecting to exercise mental superintendance, are too prone to trust to mechanical dexterity; and that some, instead of establishing the truth of conclusions on antecedent reasons, have endeavoured to prop it by imperfect analogies or mere algebraic forms. On the other hand, there are mathematicians, whose zeal for just reasoning has been alarmed at a verbal absurdity; and, from a name improperly applied, or a definition incautiously given, have been hurried to the precipitate conclusion, that operations with symbols of which the mind can form no idea, must necessarily be doubtful and unintelligible. *

I have endeavoured to establish a logic for impossible quantities; to fix the meaning of certain ambiguous expressions; and to reconcile the contradictions in the doctrine of logarithms. I indulge the hope that what I have said may deter mathematicians from attempting to found demonstration on so frail and narrow a basis as analogy; or from reposing in the dangerous notion, that there are either unaccountable paradoxes, or inexplicable mysteries, in a system of characters entirely of their own invention.

* It is to be desired, that the charges of paradox and mystery, said to be introduced into algebra by negative and impossible quantities, should be proposed distinctly, in a precise form, fit to be apprehended and made the subject of discussion.

IV. *On the Production of artificial Cold by Means of Muriate of Lime. By Mr.* Richard Walker. *Communicated by* Henry Cavendish, *Esq. F. R. S.*

Read January 22, 1801.

THE subject of the means of producing artificial cold, or the constitution of frigorific mixtures, I had considered as exhausted, in the Papers I have already had the honour to lay before this Society: so far as relates to that part of the subject which consists in generating artificial cold *without the use of ice*, it still remains so with me, having nothing new to offer.

A considerable acquisition however having been made, since my last Paper "On the best Methods of producing artificial Cold,"* by the discovery that a neutral salt, but little known, or attended to, by chemists before, produced extraordinary effects of this kind with ice, it could not fail of attracting my attention.

Since the time I first became acquainted with this circumstance, I have, as opportunity offered, been engaged in making a variety of experiments with this salt, which I flatter myself, if the Society have not already received a communication on the subject, may not prove unacceptable.

Before I relate my own experiments, it may be proper to premise a short account of those of Mr. LOWITZ, the author of the discovery.

* Phil. Trans. for 1795, p. 270.

The result of Mr. LOWITZ's experiments are, in *his* Memoir,* given according to the scale of REAUMUR; but, in *this*, are throughout reduced to that of FAHRENHEIT.

Mr. LOWITZ, Professor of chemistry in Petersburgh, having found, by an experiment made in the Winter of 1792, that caustic vegetable alkali, in a solid state, produced a degree of cold far exceeding any other substance before mixed with snow, viz. 83 degrees, determined to prosecute the subject; and, upon reflection, considering that the deliquescent salts were likely to be fittest for his purpose, fixed chiefly upon the class of muriatic salts, or those which have their base neutralized by the muriatic acid. The result of his experiments was the discovery, that crystallized muriate of lime sunk the thermometer 82 degrees; and that the other neutral salts of this class, though much inferior to that salt, exhibited nevertheless remarkable powers of the same kind.†

Professor LOWITZ, in the Memoir alluded to, observes that he has repeated my experiments with chemical salts and *snow*, but could not produce a degree of cold below + 2°. Here is evidently some mistake; for it is sufficiently known, that the novelty of my experiments depends on the production of cold, *without the use of ice* in any form.‡

Pr. LOWITZ, having found by experiment that, at the temperature of + 27°, *four* parts of muriate of lime to *three* of snow produced a temperature of − 55°, and that an increase of the

* See a translation from CRELL's Chemical Annals for 1796, by Mons. VAN MONS, Vol. XXII. p. 297, of the *Annales de Chimie*.

† Professor LOWITZ no sooner discovered the great efficacy of the muriate of lime for this purpose, than he gladly rejected the caustic vegetable alkali, on account of its burning quality; the difference being *one* degree only.

‡ See the table of frigorific mixtures. Phil. Trans. for 1795, p. 279.

salt, even in the proportion of two to one, did not diminish the effect, determined the best and *surest* proportions to be, three parts of the muriate of lime to two of snow.

But, since we shall find hereafter the temperature of $+32°$ to be a more convenient term of comparison, we may fairly state the fact thus; that muriate of lime three parts and snow two parts, mixed at the temperature of $+32°$, will give $-50°$.

The snow, to produce the greatest effect, he says, should be fresh-fallen, dry, and light or uncompressed; and the salt perfectly dry, and reduced to very fine powder.

Pr. LOWITZ's method is, to add at once the salt to the snow; the latter being previously put into a convenient vessel. As the salt produces the greatest effect whilst it retains the greatest quantity of water of crystallization, he prepares it during a freezing atmosphere, pounds it, sifts it, and keeps it in close bottles, in a cold place. With a mixture of this kind, Pr. LOWITZ froze, in one experiment, 35 pounds of quicksilver.

Pr. LOWITZ observes, that with the above precautions and management, it is impossible to fail in the design of freezing quicksilver with it.

Pr. LOWITZ found likewise, that the muriate of lime, prepared as above, produced 38 degrees of cold by solution in water; that is, by adding 3 parts of this salt, in powder, to 2 parts of water, each at the temperature of $+36°\frac{1}{2}$, the thermometer sunk to $-1°\frac{1}{2}$.

Pr. LOWITZ adds, that the muriate of lime which has been used for making frigorific mixtures, may be procured again repeatedly, as fit as at first for the same purpose, by evaporation and crystallization.*

* The muriate of lime made use of by Professor LOWITZ, in these experiments, was obtained from the residue after the distillation of caustic ammonia.

Having given an account of Pr. LOWITZ's experiments, I shall now relate briefly the result of a series of experiments made by myself, which occurred to me in consequence of Pr. LOWITZ's discovery.

My first object was, to repeat the foregoing experiment under similar circumstances; but the thermometer (the temperature of the air and materials being + 32°) sunk, in this instance, no lower than — 48°.

The liquefaction, in the above instance, was remarkably sudden; and the full effect was produced, as it were, in an instant.

Secondly, with a view either of producing a very extraordinary degree of cold, or ascertaining the point at which this salt ceases to produce further cold, I mixed the same materials, previously cooled by art to 40° below 0, when the thermometer sunk to 63° below 0.*

Thirdly, some of the same salt, in a crystallized state, was set out to deliquesce in the open air: this liquor mixed with snow, each at the temperature of + 32°, gave a cold of — 20°.

Hence it appears, that 52 degrees of heat were absorbed, or rendered latent, by the liquefaction of the snow, and 28 by that of the salt, in the first experiment; that is, in the whole, 80 degrees.

The muriate of lime used in the above experiments was prepared thus. Muriatic acid 1 part, and distilled water 3 parts, were thoroughly mixed; this liquor was then perfectly saturated with whiting, and, when clear, poured off. The mixture was afterwards evaporated, till it crystallized in air at + 32°,

* In this instance, as I afterwards found, the proportion of snow, owing in part to a considerable portion of the muriate of lime remaining frozen at the bottom of the vessel, and not mixing, was much too great.

(which happened when the liquor had been evaporated to the consistence of a thin syrup,) and then reduced to fine powder.

From the preceding account it is apparent, that Pr. LOWITZ has discovered a single frigorific mixture, by which quicksilver may be frozen whenever the temperature of the materials at mixing is no colder than + 32°; whereas, the *nitrous acid* with snow, which has hitherto been considered as the most powerful of frigorific mixtures, requires a temperature of + 7°, to produce the same effect.

At the same time, however, it should be observed, that an experiment with this salt, prepared as above, can be made only *during a freezing atmosphere;* the salt itself thus prepared, becoming, as may be inferred from the above, and as I have myself experienced, unfit for use by a warmer temperature.

Reflecting upon my former experiments, I determined to try the effect of this salt, reduced to such a strength, by evaporation, as to endure being kept, in a solid state, throughout the year. For this purpose, I found it necessary that the liquor, prepared as before, should be evaporated to the consistence of a thickish syrup, before it is set by to crystallize; when the produce will be a semi-transparent, uniform, crystalline mass, which affords, on pulverizing, a white pearl-coloured powder, which should be preserved for use in a bottle closed with a ground stopper.

Some of the powder above mentioned, (which had been previously subjected to a temperature of + 70°, without melting,) I mixed with snow, each at the temperature of + 32°; when the thermometer, to my perfect satisfaction, sunk to — 40°; and, at another time, when the air was + 20°, I froze quicksilver perfectly solid, by a mixture of the same materials.

It appeared reasonable to expect that the power of this salt for producing cold, might be assisted by the combination with, or addition of, other salts, as has been found to be the case in other instances, to a considerable degree; and, conceiving from my former experiments, that the muriate of soda and nitrate of ammonia promised the greatest success in this way, these alone producing with snow a cold of $-25°$, I was naturally led to combine these with the muriate of lime; but I obtained no advantage by this, or by any other means, worth noticing.*

In the course of last winter, and the summer preceding, I repeated my former experiments with more accuracy; and likewise made some additional ones, on the power of muriate of lime for producing cold with ice, the result of which I shall here briefly state.

In order to reduce the experiments made with the muriate of lime to a greater certainty, I carefully obtained the respective specific gravities to which this salt should be reduced by evaporation, before it be set by to cool, in order to become solid, in either instance: thus, when the muriate of lime is to be of that strength which is to be prepared and used at the temperature of $+32°$, the specific gravity of the liquor should be, at the temperature of $+80°$, 1,450; and when of that strength to be

* M. VAN MONS tried the effect of the caustic soda (which alone produces a cold of $-15°$ with snow) combined with the muriate of lime, and found the effect increased very considerably. By this means, he says, "In my new experiments on the effects of "artificial cold, I have carried it to $-53°$, old division"; (viz. 87° of FAHRENHEIT.) He does not mention the temperature at which the materials were mixed, to produce this effect.—M. VAN MONS effects a chemical union of these two salts thus: he slakes quicklime with a solution of muriate of soda; this mixture, when become cold, he filters, and afterwards evaporates, until when cold it will become solid. *Annales de Chimie*. Tom. XXIX.

kept and used at the ordinary temperature of the air at any time, the specific gravity of the liquor should be 1,490, at 80° of heat.*

The liquor, when sufficiently evaporated, should be set by to crystallize; and the crystallized mass, as soon as cold, should be reduced to very fine powder, in a glass or stone-mortar. The muriate of lime, in its solid state, being a hard brittle substance, it is necessary commonly to immerse the vessel containing it in water sufficiently warm to loosen the mass, in order to remove it out of the vessel, to pound it.

When the muriate of lime is intended to be preserved for future use, the powder should be put directly into a bottle, and closely stopped from the air; for this salt is extremely deliquescent, and hence, a dry state of the atmosphere should be chosen for preparing it.

These experiments I shall divide into two series; the first of which consists of those made with the muriate of lime prepared so as to be used in *winter* only, that is, of the strength of 1,450.

The second series consists of those made with the same salt prepared so as to be kept for use at any time, the strength of which is 1,490.

* Muriate of lime evaporated to the strength of 1,400, gives (if cooled slowly in a cold air, viz. + 20°) perfect crystals; this is the fittest state of the salt for producing cold.

SERIES I.

Exp. 1st.	Muriate of lime	3,	Snow 2,	at + 32°	gave	— 50°*
2d.	—— ——	2,	—— 1,	0		— 66°
3d.	—— ——	3,	—— 1,	— 40°		— 73°
4th.	Diluted vitriolic acid	10,†	—— 8,	— 68°		— 91°‡

* This experiment being repeated, using ice-powder, (instead of snow,) gave — 51°; ——that is, ice ground to very fine powder with the instrument described in Phil. Trans. for 1795, page 288.

† Concentrated vitriolic acid 8 parts, water 4 parts, and rectified spirit of wine 1 part, mixed and cooled previously to the temperature of the air.

‡ This experiment was made on the 10th of March last, and conducted by a series of three mixtures, thus. The materials for the *second* mixture, consisting of muriate of lime and snow, separated from each other by an intervening stratum of fine sand, were cooled in a large vessel, having a partition in the middle forming it into two compartments, to near 40° below 0, by means of a *prior* mixture of the same materials. The materials for the *second* mixture, cooled as above mentioned, were then mixed in each compartment of this double vessel, and let through an aperture at the bottom of each, (closed till then by a temporary partition,) into such an apparatus as that represented in the drawing, which contained the materials for the *third* or last mixture, consisting of the diluted vitriolic acid and snow, which had already been separately cooled in their respective vessels, to near 40° below 0.

The thermometer represented in the drawing was then placed in the tube of the upper vessel; and, when the snow was cooled to the utmost, viz. to near 70 below 0, the cooled snow was forced through into the cup containing the acid, the vessels were separated, and the snow and liquor thoroughly mixed by means of the thermometer contained in its glass tube; the thermometer was then withdrawn from the tube, and stirred about in this last or third mixture, which, in ten seconds of time, indicated a cold of 91°¼ below 0; twenty seconds more elapsed before the thermometer began to rise. The mouth of the cup in the vessel B was, in this instance, closed with waxed paper, in order that I might invert the vessel occasionally, to renew the mixture in it; and the cup itself was coated within-side with wax, in order to defend it from the action of the acid.

SERIES II.

Exp. 1st.	Muriate of lime	5,	Ice-powder	4,	at + 32°,	gave − 41°[*]
2d.	———	——— 4,	———	3,	- - + 20°,	- - − 48°
3d.	———	——— 4,	———	3,	- - + 10°,	- - − 54°
4th.	———	——— 3,	———	2,	- - − 15°,	- - − 68°

In the first experiment, the materials were mixed at the temperature of the air.

In the second, they were previously cooled, by a mixture of muriate of ammonia, nitrate of potash, and water; temperature of the air 52°; the salts which formed the preparatory mixture being recovered for use again, by evaporation.

In the third, they were cooled previously, by a mixture of muriate of ammonia, nitrate of potash, sulphate of soda, and water; temperature of the air 50°. And,

In the fourth, by a mixture of phosphate of soda, nitrate of ammonia, and diluted nitrous acid; temperature of the air 49°.[†]

Having concluded my experiments with the muriate of lime and *ice*, I proceeded to try the effects of this salt, prepared so as to retain its solid state during summer, viz. of the strength 1,490, by solution in *water;* and found that a mixture of this kind, produced twenty-nine degrees of cold; for, by adding 5 parts of the muriate of lime, in fine powder, to 4 parts of water, each at the temperature of + 50°, I obtained a cold of + 21°: this effect was not improved by the addition of other salts.

[*] The same experiment made at + 32°, with snow, (instead of ground ice,) gave − 40°.

[†] The composition and application of these frigorific mixtures are given in Phil. Trans. for 1795.

These experiments were made in such an apparatus as I am going to describe, which may at first appear new, but, upon examination, will be found to be only a different modification of the vessels represented in Fig. 3 and 4, Tab. XXIII. of Phil. Trans. for 1795; and appears to me to have all the advantages that can be obtained in an experiment of this nature.

Plate VIII. Fig. 1, represents the section of an apparatus, consisting of two vessels; viz. A A, is a vessel 4 inches in diameter, and 4 inches high, (omitting its stand, by which it rests over another vessel, presently to be described,) having a tube (of one piece with it) *a*, $\frac{7}{8}$ and $\frac{1}{2}$ in diameter, and 4 inches deep. This tube has a horizontal rim or shoulder, at $\frac{3}{8}$ of an inch from the top,* and is open at both ends; the lower one being closed occasionally, in the manner hereafter to be described; and the top of the tube, when the apparatus is of glass, by a stopple; or, if constructed of tin, by a sliding cover, fixed on the lid C.

C, is the cover or lid of this vessel, fitting over it quite close, and having a collar to fit over the tube, likewise, down to its shoulder.

B, represents a second vessel, upon which the former fits closely, but not tight. This vessel is 3 inches and $\frac{6}{8}$ in height, having a conical cup (of one piece with it) *b*, 1 inch and $\frac{1}{8}$ diameter at the top, and 1 inch in diameter at the bottom, and three inches in depth: this cup is inclosed, at the distance

* This vessel, when used, is to be filled up to this rim or shoulder *only*; that serving as a guard to prevent the frigorific mixture from getting into the tube: hence, the capacity of the vessel ending here, its height, and that of the tube, may be considered as 3 inches and $\frac{6}{8}$.

of $\frac{1}{8}$ of an inch, by a thin partition *c c*,* of one piece likewise with the vessel.

D, is the cover or lid of this vessel, fitting over it water-tight,† forming a bottom to the vessel, and having a rim $\frac{2}{8}$ of an inch deep, as a stand to insulate it from the table; the whole apparatus appearing to form, when together, one cylindrical vessel, 8 inches high, and 4 inches wide.‡ The vessels A and B contain each 1 pint and $\frac{1}{4}$; and the tube *a*, and cup *b*, 1 ounce and $\frac{1}{2}$ each.

N. B. The drawing, with the scale annexed, gives the section of this apparatus, of exactly the dimensions mentioned. The instrument described in Phil. Trans. for 1795, page 288, to

* Having ascertained by experiment, that a stratum of air of this thickness did not prevent (during the length of time which is required to freeze the water, and reduce the ice to powder in the tube *a*) the materialsin the cup *b* from receiving the temperature required, yet was nevertheless sufficient to impede the action of the mixture on the materials, when mixed in the cup *b*, during the *short time* required to take its temperature, or to freeze the quicksilver, I adopted this method, in preference to letting out the frigorific mixture from the vessel B, immediately before mixing the last materials, as formerly.

† If this cover does not fit water-tight, it may be made so, by the intervention of a thin bladder previously soaked in warm water.

‡ For the purpose of ascertaining the proper proportion of the materials to be mixed at different temperatures, and other preparatory matters, I used an apparatus of the same construction as Fig. 3, Tab. XXIII. Phil. Trans. for 1795, but differing in having two tubes instead of one. The dimensions of this apparatus, being adapted to the same scale as the former, are thus: the vessel is $5\frac{1}{2}$ inches high, and $4\frac{1}{2}$ inches in diameter; the tubes are each $\frac{7}{8}$ of an inch in diameter, and 5 inches deep. The materials, being prepared separately in this vessel, were afterwards mixed in a wine-glass.

By means of this apparatus, in one instance, muriate of lime put into one tube, in a liquid state, and water in the other, were both consolidated by cold, then ground to powder, and afterwards mixed; but the salt did not grind well, and it moreover eroded and rusted the instrument.

be used with this apparatus, should be so long as just to pass through the bottom of the tube *a*, viz. $\frac{3}{8}$ of an inch below it.

Fig. 2, represents the *spirit thermometer* made use of in the experiments: it consists of three parts, viz. A is the thermometer, having its scale-board (made of box-wood) of a *semi-cylindrical* form, being flat in front, and round at the back, in order that it may be adapted to the cylindrical tube B B, in which it slides easily up and down, centrally; and may be occasionally taken out of it. C, is a brass ferrule, cemented to, and forming one piece with, the tube, having a top or cover, which screws off and on. The scale extends from 100 below 0, to 100 above 0; the scale upwards being carried so far only as to allow of the unavoidable expansion to which the spirit may be subjected by atmospheric heat.

The thermometer (contrived by myself, and very accurately and neatly executed by Mr. NAIRNE, philosophical instrument-maker, in London,) is exactly *twice* the dimensions of the representation in the drawing, (as the scale,) and is graduated to single degrees; it has a common case, to make it portable.

N. B. A thermometer of the size described, is equally fit for an apparatus on a larger or smaller scale than that represented in Fig. 1.

The apparatus is used thus. The two vessels being taken apart, 1st. A circular piece of writing-paper is cemented* over the bottom of the tube *a*. 2d. A frigorific mixture† is made

* I use mucilage of gum arabic, or butter, if the other be not at hand; but it is sufficient merely to dip the paper in water, and apply it; the effect of the freezing mixture quickly cementing it.

† A frigorific mixture, according to the intention, may be selected from the table in page 135 of this paper; or, in defect of ice, from the table in page 279 of Phil. Trans.

in the vessel B, and the vessel covered by its lid D, then set upright, and four drams of muriate of lime put into the cup *b*. 3d. A similar frigorific mixture is made in the vessel A A, which is closed with its lid C. 4th. This vessel being placed over the other, as represented, three drams of water are to be poured gently, through a funnel, into the tube *a*, and the aperture closed. 5th. When the water is become perfectly solid ice,* the grinding instrument is to be put in, and, after suffering it to remain a short time to be cooled, the ice is to be ground gently to fine powder, (an assistant holding the apparatus firm,) and the instrument continued quite through the aperture at the bottom of the tube. 6th. The whole of the ice-powder is then to be

for 1795. The mixture I use for this purpose, is that consisting of the solution of three different salts in water; and, in order to ascertain what proportions may be necessary to fill any sized vessel, I shall give the proportional quantities for a vessel containing in measure a *wine pint*, which are as follows: of muriate of ammonia 3 ounces, nitrate of potash 3 ounces, sulphate of soda 4 ounces and ½, and water 10 ounces: having procured the salts separately in fine powder, I put these first into the vessel, and then fill up (without measuring) the vessel with water.

A mixture of this kind, made in the summer, when the temperature of the air is + 70°, will cool the materials to + 20°; and, if the salts and water are cooled to near + 50°, previously to mixing, by immersion in cold water, to + 10°. My usual method is, (without taking the usual precaution of cooling the salts,) to add the water much reduced in temperature, by pumping off a bucket or more first, by which the materials, consisting of muriate of lime and ice, are cooled to + 15° before mixing.

In winter, the experiment may be conducted by adding snow, at the temperature of the air, to muriate of lime, (cooled to a lower temperature,) in the vessel B. In summer, by adding ice-powder, cooled to a low temperature, in the vessel A, to muriate of lime cooled to + 50°, by water, (instead of a frigorific mixture,) in the vessel B.

The temperature of the springs, or of well-water, it is well known, is in this climate nearly + 50° throughout the year.

* At this period, I shake the apparatus, in order to expedite the solution of the salts, and to diffuse the effects of the freezing mixtures; or, if necessary, *renew* them.

forced into the cup of the lower vessel, and stirred about in it a little. 7th. The upper vessel being removed, and set aside, the muriate of lime and ice are to be thoroughly mixed, and a small tube, containing the quicksilver to be frozen, stirred about in the mixture; or the bulb of a spirit-thermometer, to take its temperature, which, if the experiment be conducted properly, will indicate, even if the experiment be made in summer, a cold of $-50°$.*

The apparatus, as represented in the drawing, is upon as fit a scale as may be required for common experiments; by attention, however, to the proportions given, one of any size may be procured.

Muriate of lime produces no effect upon tin or japanned vessels; hence the apparatus is best made of common block-tin; observing that the tube and cup be made of the *thinnest* tinned iron, and the whole besides of the same substance, but considerably *thicker*.†

* Muriate of lime and ice-powder, mixed at the temperature of $+20°$, give a cold of $-48°$; if mixed at $+15$, of $-51°$; and, at $+10°$, a cold of $-54°$.

The freezing point of quicksilver is $-39°$; but that metal requires a temperature of $-45°$, to assume its perfectly solid state.

I have repeatedly frozen quicksilver in the middle of summer, by mixing together muriate of lime and ice-powder, at $+20°$; and likewise, by mixing together nitrous acid and ice-powder, at $+8°$.

† The best method of constructing vessels for the purpose of excluding heat, is obviously to have them made of the best non-conducting substance, lined within with the best conducting substance; hence these vessels (the tubes excepted) might be fitter for the purpose, if made of wood lined with tin.

My general rule for constructing the apparatus, is to allow *four* times, by measure, the water to be frozen and reduced to powder, in the capacity of the tube that is to contain it; and *three* times the weight (by measure) of the muriate of lime, to the cup in which the muriate of lime is to be cooled, and the ice-powder afterwards added

The tube and cup should be very smooth within-side, and perfectly central; the tube having as little seam as possible, that the grinding instrument be not obstructed.

The grinding instrument acts best when the edge, instead of being quite horizontal, is a little inclined from each shoulder, towards the centre.

In order to keep this Paper within tolerable limits, I have carefully avoided a repetition of all matters mentioned in my former Papers on this subject; I must therefore refer to those, especially that "On the best Methods of producing artificial Cold,"* for the particular mode of conducting experiments on cold; this being essentially the same in principle, whatever be the materials made use of to effect it. Hence, the apparatus just described is applicable to the use of the *mineral acids*,† as well as to that of *muriate of lime;* recollecting that it is necessary to substitute glass for tin, when the former are used; or to give the inside of the cup, or vessel containing it, a coating of wax, to defend the tin from their action.

Having given an account of Pr. LOWITZ's experiments on the power of muriate of lime for producing artificial cold, and added such observations of my own as resulted from them, I

to it: and, when nitrous acid is used, (instead of muriate of lime,) *four* times its weight; and about four times the diameter of the vessels to that of the tube.

The preparatory mixtures, that is, such as are used for cooling the materials previously to mixing, are best made of *ice* and salts; these retaining their temperature longer than those consisting of solutions of salts in water or acid; but, in either case, if necessary, they may be occasionally renewed, after the water is become solid.

* Phil. Trans. for 1795. p. 270.

† Nitrous acid, and vitriolic acid, may at any time be immediately procured from a chemist's shop; whereas the muriate of lime, not being used for any other purpose,

shall conclude by exhibiting a general view of the different frigorific mixtures composed of chemical substances with *ice*, as I have before done, (Phil. Trans. for 1795, page 279,) of those in which the use of ice is dispensed with.

Class I.

Acids and salts.	Ice.	Temp. of mat. before mixing	Temp. or cold produced
Muriate of soda 1,	Snow	2,	— 5°
—— —— 2, Muriate of ammonia 1,	——	5,	— 12°
—— —— 10, —— —— 5, Nitrate of potash 5,	——	24,	— 18°
—— —— 5, Nitrate of ammonia 5,	——	12,	— 25°

Class II.

Diluted vitriolic acid 2,*	Snow† 3,	+32°	—23°
Concentrated muriatic acid 5,	—— 8,	+32°	—27°
Concentrated nitrous acid 4,‡	—— 7,	+32°	—30°
Muriate of lime 5,‖	—— 4,	+32°	—40°
—— —— 3,¶	—— 2,	+32°	—50°
Caustic vegetable alkali 4,	—— 3,	+32°	—51°

must commonly be prepared for this *alone*; hence it may not unfrequently happen that the former, on this account, may be preferred.

* Concentrated vitriolic acid, diluted with half its weight of snow, or distilled water, and cooled.

† Snow that is fresh, dry, and uncompressed, or such as has never been subject to the effects of a temperature less than freezing; or, when such is not to be procured, ice reduced to powder, in the manner described in Phil. Trans. for 1795, p. 271, may be substituted in its stead, with equal effect.

‡ Concentrated *fuming* nitrous acid alone; or concentrated *pale* nitrous acid, diluted with one-fifth its weight of snow, or distilled water, and cooled.

‖ Of the strength of 1,490, at 80° of heat.

¶ Of the strength of 1,450, at 80° of heat.

The above table is divided into two classes. The first class consists of mixtures of salts and ice, in which the temperature of mixing is of no consequence, the effect produced being the same at any temperature of the air: the salts should be in the state of powder. Ice pounded small may be substituted, with equal effect, for snow.

The second class consists of such mixtures as will produce an effect *greater*, the colder the temperature is at which the materials are mixed, but in a *diminishing* ratio; ceasing entirely at that degree of cold at which the composition itself freezes.* The salts should be in the state of fine powder.

N. B. The figures after the salts, or acids, and ice, express the proportions, *by weight*, to be used.

In the above table, the ordinary effect of snow, or ice-powder, is given; but, if the latter be prepared (ground) with a sharp instrument, using light pressure, the effect will be somewhat greater, the ice being then reduced to an impalpable powder: hence, ice powder, thus obtained from a block of ice, may at any time be substituted for snow.

Cold is produced by mixing various other chemical substances with ice: in the above table, such only are retained as produce that effect in a remarkable degree.

As the new nomenclature is now generally adopted, I have used it in this paper.

* The materials may be cooled, previously to mixing, when required, by a frigorific mixture taken from the table: for this purpose, either of the mixtures in Class I. are convenient; particularly the first, consisting of muriate of soda and snow.

POSTSCRIPT.

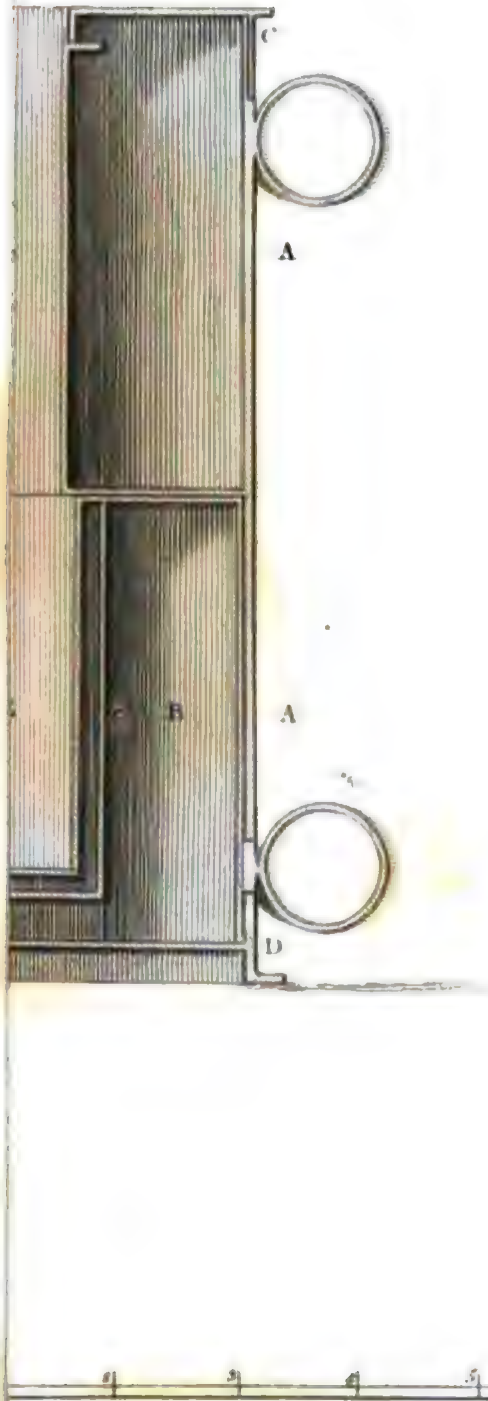
In the course of my former papers on the subject of cold, I have had occasion to make, incidentally, some remarks on the power of water, under certain circumstances, in resisting an extraordinary degree of cold without freezing; likewise on the particular kind of agitation which induces water, cooled below its freezing point, to crystallize or become ice.* As these are subjects which have likewise engaged the attention of others, I shall take the liberty of barely mentioning a fact, having relation to those points, which has lately occurred to me.

It is a remarkable circumstance respecting artificial freezing, that the ice thus procured in the usual way, (viz. by immersing the water to be frozen, in a convenient vessel, in a frigorific mixture,) will always be more or less opake, never transparent: this I had constantly remarked, without much attending to it; however, having in the course of last summer been induced to try the effect of an ice-speculum for producing heat, it became necessary that the ice, which in this instance was substituted for glass, should be perfectly transparent. After varying the process in every possible way I could think of, by *immersing the water* to be frozen, without effect, I at last succeeded completely, by forming a coating of ice, of sufficient thickness, on the outside of a vessel containing the frigorific mixture; the bottom of this vessel, which was made concave for this particular purpose, being immersed for a sufficient length of time in a shallow pan of water.

* Phil. Trans. for 1788, page 401.

Hence arises the means, before unknown to me, of obtaining ice, either in an opake, or perfectly transparent state; moreover, water, as I have experienced lately, constantly forms a coating of ice on the outside of a vessel containing a frigorific mixture, so soon as it is cooled to $+32°$.

1.



Scale of Inches.

Fig. 2

V. *Account of a monstrous Lamb. In a Letter from Mr.* Anthony Carlisle, *to the Right Honourable Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read January 29, 1801.

Dear Sir,

I am much indebted to you for the privilege of inspecting the monstrous lamb sent by Dr. Pulteney of Blandford, whose laudable interest for the promotion of science, induced him to present it to you. The animal is a male, and apparently at the full period of gestation: its whole frame, excepting the head, is of the natural structure; the deviation in structure of this part, resembles none of the series of monsters which are usually met with among complicated animals. I have preserved the entire skin, in hopes of retaining the outward peculiarities of this creature. According to your suggestion, I took the opportunity, while the subject was in perfect preservation, to examine the brain and its connections: they seem to me very remarkable, and might have afforded matter, to an acute observer, of high interest in the science of physiology, had this monster been yeaned alive. Perhaps it may not be altogether unuseful, to record those internal deviations from the ordinary structure which this dissection presented; and the suggestions arising may possibly excite the attention of some future observer, who may be more happily circumstanced.

The head is disproportionably small; there being no other resemblance to the natural form than in the external ears,

which are brought together by their insertions in the front part of the head: the apertures called by anatomists meatus externi, are wanting. Immediately between the insertion of the ears, an opening presents itself, lined with cuticle, and capable of receiving a bougie, the size of the human male urethra: this proved to be the common passage to both the œsophagus and the trachea. The outer surface of the head is regularly clothed with wool; and there are no appearances of abrasion, or mechanical injury, having taken place at an early period of its formation; such as are observable in monsters, and perhaps sometimes produced by the rubbing of the umbilical cord, or by the contiguity of the uterine contents, whilst the young animal is yet in a soft state. See Plate IX. Fig 1, and the references.

The whole of the organs which are naturally found in the face, are, in the present instance, defective: no vestiges of the eyes, the nose, or any of the apparatus belonging to the mouth, are to be seen. The cranium is perfectly formed into hard bone, and bears a near resemblance to the head of a tortoise; it is about the size of a plover's egg. The os hyoides, and its processes, are in the natural state: there is no other part of the tongue. The cartilages of the larynx, together with their muscles and vessels, are in their places, saving the epiglottis, which is joined in with the common aperture, making a cartilaginous ring. The bones, muscles, blood-vessels, and nerves of the neck, are natural. Under the skin which lies between the cartilaginous insertions of the outward ears, there is a small depression in the skull, in which are lodged three regularly formed bones, of a tooth-like structure, immersed in a gelatinous substance, like that which is found in the bony part of very young growing

teeth: from the shape and size of these bones, added to their situation and the want of enamel, I take them to be portions of the ossicula auditus, run together, and united into masses. See Fig. 3. The internal surface of the cranium, is neatly lined with the dura mater: it is deficient in all the processes which divide the different portions of the brain. The anterior limits of this cavity terminate at the hinder part of the sulci; where the middle lobes cerebri ought to be lodged. The internal carotid arteries, and the pituitary gland, are missing. The two vertebral arteries enter the skull as usual, and form the basilary artery; which soon divides itself again, for the supply of the pia mater and brain of this monster. The pia mater envelopes the brain, as is usual, and is unconnected with the dura mater; these membranes being each of them smooth, loose, and natural. I was surprised to find the whole cerebrum, and all its nerves, deficient: the cerebellum disposed quite orderly, and the following pairs of nerves nearly in their natural situations. First, a large pair, at the anterior inferior part, which is analogous to the crura cerebri: these seem to stand in the place of the sixth pair, only that their whole substance terminates in the upper cervical ganglion of the intercostal chain of nerves. Secondly, a large double pair, analogous to the seventh, coming out at the tuberculum annulare, and penetrating the meatus auditorius internus: the portio dura of this double pair appears on the side of the neck, after its exit from the cranium; the portio mollis remains in the labyrinth of the organ of hearing. From the sides of the medulla oblongata, immediately at its origin, a number of separate fibrils come out, which are joined into one common chord, becoming the par vagum, and being finally dispersed in the ordinary manner. The

accessorius to the par vagum, comes up from the medulla spinalis, and takes its departure with its usual companion. Not a vestige of any other nerve appears in the cranium; the cavity being perfectly filled, and all the parts free from diseased appearance. See Fig. 4. The vena magna GALENI, or a vein analogous to it, passes down the hinder part of the cerebellum, into the lateral sinuses, which are the only receptacles for the returning blood; and all the veins of the pia mater open directly into these sinuses. Upon cutting through the centre of the brain, longitudinally, the intermixture of the cortical and medullary substances form the appearance called Arbor vitæ, in a perfectly natural state; the texture of those two substances being firm and natural. The fourth and only ventricle is unusually large; and the portio mollis of the seventh pair of nerves arises from its inside, as is usual. The anterior rounded ends of the crura cerebri, present a mass of healthy medullary substance. The outer surface of the cerebellum is divided into the parallel layers or folds, and those vermiform processes, which distinguish its general character.

The narration of these appearances assists and confirms other facts, in demonstrating, that the formation and growth of animals in the uterus, are independent of any influence from those parts of their brain which properly belong to sensation. We have to regret, that this animal did not live to shew the phenomena of volitions directed to its limbs, and other apparatus, without that intelligence from the organs of the senses which regulates and directs the efforts of perfect animals. The careful observance of such circumstances may, in future, bring us to discoveries of the highest value, in that part of physiology which is now enveloped in deep mystery: the facts at

present collated are not sufficient. The intellectual phenomena of persons who sustain known injuries of particular parts of the brain; the appearances on the dissection of ideots, with their mental particularities; the anatomical history of maniacs, all promise, when properly cultivated, a series of truths, which it may not be extravagant to hope, will open sublime views into those recesses of our construction which justly rank among the most curious, if not the most important objects of research. Returning you many thanks for the favour you have done me, by committing this inquiry to my hands,

I am, &c.

ANTHONY CARLISLE.

March 28, 1800.

REFERENCES TO THE FIGURES. SEE PLATE IX.

Fig. 1. A front view of the head, and part of the neck, of this monster.

a, *a*, The external ears.

b, The common aperture of the larynx and œsophagus.

Fig. 2. The naked skull, and part of the neck dissected.

a, The second vertebra of the neck.

b, The œsophagus.

c, The trachea.

d, The common opening of the œsophagus and larynx.

e, The situation of the small bones, supposed to be the rudiments of the ossicula auditus.

f, The horn of the os hyoides.

g, The body of the os hyoides.

h, The anterior part of the skull.

Fig. 3. The three pieces of bone, shewn in Fig. 2, letter *e*.

Fig. 4. The side view of the brain, and its nerves.

a, The beginning of the medulla spinalis.

b, The par vagum, with the accessorius nerve.

c, The portio mollis and dura of the auditory nerve.

d, The supposed sixth pair.

e, Part of the basilary artery.

Fig. 1.

Fig. 2.

Fig. 3.

VI. *An anatomical Description of a male Rhinoceros. By Mr.* H. Leigh Thomas, *Surgeon. Communicated by* George Fordyce, *M. D. F. R. S.*

Read January 29, 1801.

Our knowledge of this animal has hitherto been extremely limited, both with regard to its natural history, and also its internal structure. A paper by James Parsons, M. D. giving a very accurate description of a young rhinoceros, was read before the Royal Society, in June, 1743; but, as the Doctor does not attempt to describe more of it than the external figure and coverings, (which are delineated,) we may presume that he never had any opportunity of examining the internal parts: his account, however, as far as it goes, is in every respect correct; I shall not, therefore, take up the time of this learned Society by a useless recapitulation, but proceed to describe such appearances as have not yet been noticed.

The subject of the following observations was brought from the East Indies to England, where it was intended he should remain, until a favourable opportunity should offer of sending him to Vienna. During the passage from India, he appeared to enjoy a good state of health, which continued uninterrupted, until a few days before his death; at which time, he was attacked with difficulty of breathing, and died before he had attained his third year. In the course of this time, he had become perfectly docile and tame; but never, by

actions or otherwise, expressed the smallest regard or affection for his keeper, or for any of the people who occasionally fed him; neither was he easily irritated, but preserved, on all occasions, the most perfect indifference and stupidity. He was fed chiefly upon hay and oats, also potatoes, and other fresh vegetables; his consumption of which was prodigious, exceeding that of two or three working horses. It would appear, that this animal had not arrived to near its full growth: he was scarcely so high as a two year old heifer; but the bulk of his body, by measurement, considerably exceeded the length. The horn, which is affixed to the upper lip of the adult rhinoceros, was here just beginning to sprout. The hoofs were divided into three obtuse parts: the soles of the feet were well defended, by a large mass of elastic matter, covered by a strong horn-like substance.

It will not be necessary to give a minute detail of the anatomy of this animal; it is only requisite to remark appearances which are peculiar to it, and such as are not commonly met with in other quadrupeds: in general, the structure may be said to correspond to that of the horse; the peculiarities, however, shall be briefly noticed.

The skin, it is well known, is extremely hard and tuberculated; though smoother, and easily cut through by a common knife, on the under parts of the body: a considerable degree of sliding motion was observable between it and the surface underneath; this arose from the great quantity of loose cellular membrane, deposited between them, for the purpose of allowing the hard skin a power of accommodating itself to the body, when in a recumbent position. I could not observe any fibres corresponding to the panniculus carnosus, generally

found in quadrupeds: indeed this muscle would have been useless here; for, from the structure of the skin, the animal could not be sensible to the bites of insects; nor could so weak a power act upon a substance so strong and inelastic. The abdominal muscles were exceedingly strong, and well marked: the tendinous fasciæ were much thicker than I had ever observed in any other animal; obviously to give a sufficient support to the great weight of the viscera. The incisor teeth were only four in number; two situated in each jaw; these are placed a considerable distance from each other: besides them, I observed in the head of another rhinoceros, five years old, and where the soft parts had been removed, two smaller teeth, placed one on each side those of the lower jaw; these were not pointed. There were only eight of the molares, in each jaw: this number, of course, would be increased, as the growth of the maxillary bones advanced; their form may be considered as peculiar, and has been already noticed by Mr. Home, in the Philosophical Transactions for 1799, Tab. XXI. The inside of the mouth presented nothing unusual; the membranes covering it were not thicker than those found in other graminivorous animals. The pharynx and œsophagus were large and capacious. The stomach, with the whole of the alimentary canal, was, in external appearance, very similar to that of the horse, only that the cæcum was considerably larger; which variety accounts for the great size of the abdomen, already noticed. The stomach, upon its inside, was in every part covered by a secreting surface; whereas, in the horse it is partly cuticular. The small intestines were extremely short; but the surface upon the inside was considerably extended, by the internal coat being thrown into processes of an oblong form; these, after the

mesenteric vessels were injected, put on a beautiful villous appearance: it would appear, that they answer the same purposes as the valvulæ conniventes in the human subject; they differ only in the mode of arrangement, and are unlike what I have ever observed in any other animal.

The liver was of a dark black colour, very soft, giving as little resistance to pressure as the human spleen generally does: it was divided into several lobes. The gall bladder was wanting. The spleen and pancreas were very similar to those of the ox. The kidnies were large, and considerably flattened: they were lobulated, but their lobes did not appear so distinct as those of the same gland belonging to the bear; probably, as the animal advances in life, this appearance may be altogether lost, as takes place in the human body, and a variety of other animals. Upon throwing some size, coloured with vermillion, into the emulgent artery, I was surprised to perceive the coloured matter escape by the ureter, without any considerable pressure of the piston: this circumstance induced me to insert the pipe into the excretory duct of the other kidney; when the injection escaped, with the same ease, by both artery and vein. I should not have noticed these circumstances, which have occasionally occurred to me when injecting the human kidneys, and also those of other animals; but, in these instances, the great facility with which the injection passed, surprised me, and at the same time proved, in a remarkable manner, the simple structure of this gland. The organs of generation had not arrived to maturity: the testes were small, and situated without the abdomen; the vasa deferentia did not allow quicksilver to pass along them; and, upon the whole, it was evident the testes never had secreted. The vesiculæ seminales were cellular;

and in shape and structure like those in the human subject: they contained only a small quantity of a ropy fluid. Upon throwing some coloured wax into the corpora cavernosa of the penis, the extremity became expanded, with the meatus urinarius placed in the centre; this expansion was not so considerable as is observed in the horse: about three inches below, a second enlargement took place, though not so compleat and perfect as the first. The penis was curved in its form, with the convex side towards the body; which proves that this animal must be a retro-coient: indeed his general structure might have suggested this idea, had not these parts been particularly attended to. The contents of the thorax presented nothing worthy of remark. The lungs every where adhered to the inside of the thorax, and were in a high state of inflammation; which latter circumstance was probably the cause of the animal's death.

Whilst the animal was living, the eyes always appeared dull and watery; the upper and under palpebræ were scarcely ever observed to come together; the palpebra tertia was frequently carried over the cornea, and corresponded in shape and structure to that of the ox. The muscles of the eye-ball were exactly similar to those of other graminivorous animals: the globe of the eye was not larger than that of the sheep; and the cornea was much smaller. Upon cutting through the sclerotic coat, it was found somewhat harder and thicker than what is observed in the sheep; and, upon endeavouring to separate it from the choroid, I found an uncommon resistance at the posterior part of the eye; though in other parts, the adhesion between the coats appeared less than what takes place in the human body. This unusual connection, naturally directed my attention more particularly towards it; when I readily discovered four

processes, arising by distinct tendons from the internal and posterior portion of the sclerotica, and at equal distances from the optic nerve. These processes passed forwards between the coats, gradually becoming broader, and being insensibly lost in, and forming a part of, the choroid, at the broadest diameter of the eye: the connexion between the coats around the outer circle of the cornea, was the same as is observed in the eye of other animals. The processes had a muscular appearance; the fibres running forward, in a radiated direction; they were detached from the coats with the greatest facility, except at their origins and insertions, where it required considerable force to tear them from the sclerotica; and, at their terminations, they became so intimately connected with the choroid, as to form only one substance. On neither of their surfaces was there any thing similar to the nigrum pigmentum; the pigment was confined to the inside of the choroid coat, without any structure similar to the tapetum lucidum. The ciliary processes were affixed to the crystalline lens; they were extremely short, and indistinct; not having that beautiful arrangement commonly seen in the eye of other quadrupeds. The iris was circular, and of a dark brown colour. The crystalline lens was somewhat remarkable with respect to its form, being nearly spherical; this was very strongly marked, when compared with the lenses of several other animals; the anterior surface was a little flattened.

The peculiarities already observed in the structure of the eye, in different animals, are very numerous; but I do not know that the variety above stated has hitherto been noticed by any one: the structure of the processes, as far as the sight can determine, appears to be muscular; and, what more particularly tends to confirm this notion, is the very distinct tendons con-

necting them with the sclerotic coat. It is well known that the iris, and also other parts of the body, possess to a great degree the power of contraction, without our being able to demonstrate muscular fibres; allowing, therefore, these processes to have the common properties of muscles, we shall be better enabled to form some idea of their uses.

It is related by naturalists, that the rhinoceros does not enjoy a very quick sight; and that he can only distinguish objects which are placed immediately before him. This notion most probably has arisen from the apparent dullness of his eyes, and the great difficulty he must meet with in turning the head from side to side, encased as the neck is by its strong unyielding coverings. I conclude, however, that if we should ever become acquainted with the natural habits of this animal, his vision will be found to be as perfect as that of any other of the same class. In the muscles, I have already remarked, that there is no difference; of course, the eye-ball, with those powers, must enjoy the common motions. With respect to his ability for seeing near objects, it is not probable that nature should have denied to this creature, a faculty which has been granted to every other, viz. a power of minutely examining their food before it is taken into the stomach; now, as his eyes are placed nearer the mouth than in any other quadruped we are yet acquainted with, it is but reasonable to suppose, that his powers for accommodating vision to very near objects must be equal, if not superior, to theirs. In the easy and natural state of the eye, it is probably so adjusted as to view with perspicuity very near objects, requiring some change to adapt it for distinguishing distant ones. This change, most likely, is effected by the four processes acting conjointly: at their terminations they completely encircle the eye

at its broadest diameter; therefore, upon their contracting, the axis of vision will be shortened, and the retina brought nearer to the crystalline lens; consequently, the eye will be better fitted for seeing objects at a distance. In birds, there is placed at the posterior part of the eye, a muscular process, called by HALLER, pecten avium, by others, marsupium: this answers the same purposes as these processes, the arrangement of its fibres only differing. In the chameleon, and also in many fishes, a similar structure is found, calculated to produce the same effects; and probably something of the same nature may be seen in the eyes of many other animals, which has hitherto escaped observation.

As it is impossible for language to convey a just idea of the relative situation of these processes, I have subjoined sketches, shewing them in three different points of view: the parts represented are of the natural size.

Plate X. Fig. 1, Represents a longitudinal section of the globe of the eye; the vitreous humor is removed, and the choroid coat detached and brought forward: a bristle is introduced between the two processes and the sclerotic coat.

Fig. 2, Represents the internal and posterior portion of the sclerotic coat; the foramen for the passage of the optic nerve in the centre; and the four processes arising at equal distances from it.

Fig. 3, Represents an outside view of the processes, losing themselves in the choroid coat: portions of folded paper are placed under each, to render them more distinct.

Fig. 4. A portion of the jejunum inverted, to shew the foldings of its internal membrane.

Fig. 2.

Fig. 3.

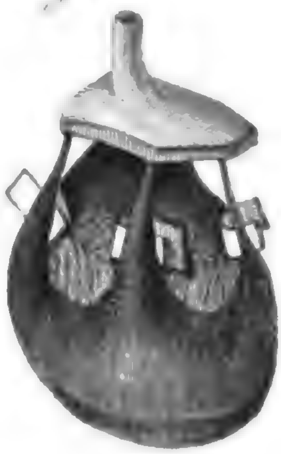


Fig. 1.

VII. *Demonstration of a Theorem, by which such Portions of the Solidity of a Sphere are assigned as admit an algebraic Expression. By* Robert Woodhouse, *A. M. Fellow of Caius College, Cambridge. Communicated by* Joseph Planta, *Esq. Sec. R. S.*

Read February 12, 1801.

IN 1692, a problem was proposed by VIVIANI, (Act. Erud. Lips.) to the geometricians of his time, in which it was required to separate from the surface of a sphere, such portions, that what remained should be quadrable.

In the second volume of the Memoirs of the National Institute, M. BOSSUT announces a theorem relative to the solidity of a sphere, very simple, he says, and as remarkable as VIVIANI's, but depending on an integration much more complicated: the theorem is this.

" If a sphere be pierced perpendicularly to the plane of one of its great circles, by two cylinders of which the axes pass through the middle points of two radii that compose a diameter of this great circle, the two portions, thus taken away from the whole solidity of the sphere, leave a remainder equal to two-ninths of the cube of the sphere's diameter."

M. BOSSUT withholds the analysis that led to this result. I have obtained it in the subjoined process, in which the integration is not at all more complicated than what is used in the solution of the Florentine problem.

Let x, y, z be three rectangular co-ordinates, that determine the position of any particle of a solid body relatively to any three fixed axes, then the element of the solidity may be represented by the paralellopiped $\dot{z}\dot{y}\dot{x}$; hence, s (solidity) $= \int\!\int\!\int \dot{z}\dot{y}\dot{x}$, which triple integral must be taken first relatively to z, supposing y and x constant; next, relatively to y, supposing x constant, having previously substituted in the function of z, for z, its value in terms of x and y; and, lastly, relatively to x, having previously substituted in the function of y, for y, its value in terms of x. The order of these integrations may be varied at pleasure.

In the present case, let the origin of the co-ordinates be the centre of the sphere; then $s = \int\!\int\!\int \dot{z}\dot{y}\dot{x} = \int\!\int (z' - z)\,\dot{y}\dot{x}$; and, if the integral be taken from the plane of x and y, where $z = 0$, to the surface of the sphere, where $z = z'$, $s = \int\!\int z'\dot{y}\dot{x}$; but the equation to the surface of the sphere is $z' - \sqrt{r^2 - x^2 - y^2} = 0$; hence, $s = \int\!\int \sqrt{r^2 - x^2 - y^2}\,\dot{y}\dot{x}$.

Without the aid of some transformation, it would be extremely difficult to find the value of this double integral: the transformation is to be effected in the following manner.

In the expression $z'\dot{x}\dot{y}$, suppose first $x =$ funct. $(y, \varrho) = \mathrm{R}$; then, regarding y as constant, $\dot{x} = \left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)\dot{\varrho} \therefore z'\dot{x}\dot{y} = z'\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)\dot{\varrho}\dot{y}$; and, when its integral is to be taken relatively to ϱ, the value of $x = \mathrm{R}$ must be substituted for x in z' or $\sqrt{r^2 - x^2 - y^2}$.

Again, let $y =$ funct. $(\varrho, \theta) = \mathrm{Q} \therefore$ considering ϱ constant, $\dot{y} = \left(\frac{\dot{\mathrm{Q}}}{\dot{\theta}}\right)\dot{\theta}$; hence, $z'\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)\dot{\varrho}\dot{y} = z'\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)\dot{\varrho}\left(\frac{\dot{\mathrm{Q}}}{\dot{\theta}}\right)\dot{\theta}$; and, to integrate, substitute for y its value (Q) in $z'\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)$. The expression $z'\dot{x}\dot{y}$ is thus transformed into another, relative to two variable

quantities, ϱ and θ; and, since we have $x = \mathrm{R} = \text{funct.}\ (y, \varrho)$, $y = \mathrm{Q} = \text{funct.}\ (\varrho, \theta)$, x and y may be deduced functions of ϱ and θ. Hence,

$$\left.\begin{aligned} \dot{x} &= \left(\frac{\dot{x}}{\dot{\varrho}}\right)\dot{\varrho} + \left(\frac{\dot{x}}{\dot{\theta}}\right)\dot{\theta} \\ \dot{y} &= \left(\frac{\dot{y}}{\dot{\varrho}}\right)\dot{\varrho} + \left(\frac{\dot{y}}{\dot{\theta}}\right)\dot{\theta} \end{aligned}\right\} \left(\frac{\dot{x}}{\dot{\varrho}}\right)\dot{\varrho},\ \left(\frac{\dot{x}}{\dot{\theta}}\right)\dot{\theta}\ \text{\&c. being partial fluxions of } x \text{ and } y,$$

to determine $\left(\frac{\dot{\mathrm{Q}}}{\dot{\theta}}\right)$ and $\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)$, $\left(\frac{\dot{\mathrm{Q}}}{\dot{\theta}}\right) = \frac{\dot{y}}{\dot{\theta}}$, and $\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)$ was made equal to $\left(\frac{\dot{x}}{\dot{\varrho}}\right)$ on the supposition that y was constant, or that $\dot{y} = 0$; hence, $\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)$ must be determined from the equations $\dot{x} = \left(\frac{\dot{x}}{\dot{\varrho}}\right)\dot{\varrho} + \left(\frac{\dot{x}}{\dot{\theta}}\right)\dot{\theta}$

and $0 = \left(\frac{\dot{y}}{\dot{\varrho}}\right)\dot{\varrho} + \left(\frac{\dot{y}}{\dot{\theta}}\right)\dot{\theta}$,

eliminating $\dot{\theta}$, $\left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right) = \frac{\dot{x}}{\dot{\varrho}} = \dfrac{\left(\frac{\dot{x}}{\dot{\varrho}}\right)\left(\frac{\dot{y}}{\dot{\theta}}\right) - \left(\frac{\dot{x}}{\dot{\theta}}\right)\left(\frac{\dot{y}}{\dot{\varrho}}\right)}{\left(\frac{\dot{y}}{\dot{\theta}}\right)}$ $\therefore \left(\frac{\dot{\mathrm{R}}}{\dot{\varrho}}\right)\left(\frac{\dot{\mathrm{Q}}}{\dot{\theta}}\right)$ $= \left(\frac{\dot{x}}{\dot{\varrho}}\right)\left(\frac{\dot{y}}{\dot{\theta}}\right) - \left(\frac{\dot{x}}{\dot{\theta}}\right)\left(\frac{\dot{y}}{\dot{\varrho}}\right)$. Hence, $z'\,\dot{x}\,\dot{y}$ is transformed into $\left(\left(\frac{\dot{x}}{\dot{\varrho}}\right)\left(\frac{\dot{y}}{\dot{\theta}}\right) - \left(\frac{\dot{x}}{\dot{\theta}}\right)\left(\frac{\dot{y}}{\dot{\varrho}}\right)\right)\dot{\varrho}\,\dot{\theta}$.

The preceding transformation is general, whatever functions of ϱ and θ, x and y are. To obtain a particular solution in the present case, let the co-ordinates x and y be transformed into a radius vector ϱ, and an angle θ, which ϱ makes with x;

$$\therefore\ x = \varrho \cos.\theta$$

$$y = \varrho \sin.\theta \text{ and } \sqrt{x^2 + y^2} = \varrho$$

$$\therefore \left(\frac{\dot{x}}{\dot{\varrho}}\right) = \cos.\theta,\ \left(\frac{\dot{x}}{\dot{\theta}}\right) = -\varrho \sin.\theta$$

$$\left(\frac{\dot{y}}{\dot{\varrho}}\right) = \sin.\theta,\ \left(\frac{\dot{y}}{\dot{\theta}}\right) = \quad \varrho \cos.\theta$$

$$\therefore \left(\frac{\dot{x}}{\dot{\varrho}}\right)\left(\frac{\dot{y}}{\dot{\theta}}\right) - \left(\frac{\dot{x}}{\dot{\theta}}\right)\left(\frac{\dot{y}}{\dot{\varrho}}\right) = \varrho\left(\overline{\cos.\theta}^2 + \overline{\sin.\theta}^2\right) = \varrho;$$

and, since $z' = \sqrt{r^2 - x^2 - y^2} = \sqrt{r^2 - \rho^2}$, $z'\dot{x}\dot{y}$ is transformed into $\dot{\rho}\rho\dot{\theta}\sqrt{r^2 - \rho^2}$; hence $s = \pm\iint \rho\dot{\rho}\dot{\theta}\sqrt{r^2 - \rho^2} = \int\left(\frac{(r^2-\rho^2)^{\frac{3}{2}} - (r^2-\rho'^2)^{\frac{3}{2}}}{3}\right)\dot{\theta}$.

Let the integral commence from the circumference of the circle in the plane of which x and y are situated; then $\rho = \rho' = r$, and $s = \int\frac{(r^2-\rho^2)^{\frac{3}{2}}}{3}\dot{\theta}$, in which expression $\frac{(r^2-\rho^2)^{\frac{3}{2}}}{3}\dot{\theta}$ is the element of the solidity insisting on that part of the plane of x which is included between the circumference of the circle whose radius is r, and the curve whose nature is determined by the relation existing between x and y, or ρ and θ.

In the present problem, this latter curve is given to be a semicircle described on r as a diameter; hence, as ρ is a line drawn from an extremity of this diameter to the circumference, making an angle θ with the diameter, we have, by similar figures, tang. θ $(t) = \frac{\sqrt{r^2-\rho^2}}{\rho}$, but $\dot{\theta} = \frac{\dot{t}}{1+t^2} = \therefore \frac{-\dot{\rho}}{\sqrt{r^2-\rho^2}}$.

*Hence, $s = \pm\int\frac{(r^2-\rho^2)^{\frac{3}{2}}}{3}\dot{\theta} = \pm\int\frac{(r^2-\rho^2)}{3}\dot{\rho} = \frac{r^2\rho}{3} - \frac{\rho^3}{9} + C$ $\therefore s = \frac{2r^3}{9} - \frac{\overline{3r^2\rho + \rho^3}}{9}$, when $\rho = 0 = \frac{2r^3}{9}$, the value of the portion of the solid insisting on a base in the plane of x and y, and the bounding lines of which are a quadrantal arc, (rad. r) a semicircle, (diam. r) and a radius (r) perpendicular to the diameter

* If $\dot{s}$ is expressed in terms of t and $\dot{t}$, the integration is less simple; for $\dot{s} = \frac{r^3 t^3 \dot{t}}{3(1+t^2)^{\frac{5}{2}}} = \frac{r^3}{3} \times t^3\dot{t}\,(1+t^2)^{-\frac{5}{2}} = -\frac{r^3}{9}$ fluxion $\left(t^2(1+t^2)^{-\frac{3}{2}}\right)$ $+ \frac{2r^3}{9}t\dot{t}\,(1+t^2)^{-\frac{3}{2}} \therefore s = -\frac{r^3}{9}\frac{t^2}{(1+t^2)^{\frac{3}{2}}} - \frac{2r^3}{9} \times \frac{1}{(1+t^2)^{\frac{1}{2}}}$ + correction $= \frac{2r^3}{9}$, when the integral is taken from $t = 0$ to $t = \infty$.

of the semicircle: hence, for the whole sphere, $8\,s = \frac{8 \times 2 r^3}{9}$ $= \frac{2}{9} \times (2\,r)^3 = \frac{2}{9}$ (diameter)^3, which is the result the theorem announces.

1. The transformation* of $z' \dot{x} \dot{y}$ into $z' \left(\frac{\dot{R}}{\dot{\varrho}}\right) \left(\frac{\dot{Q}}{\dot{\theta}}\right) \dot{\varrho}\,\dot{\theta}$, in the present case, may be avoided by originally taking the element of the solidity differently; thus, if $\dot{\alpha}$ be the incremental arc described with radius ϱ, the element of the solidity $= z' \dot{\alpha}\,\dot{\varrho} =$ (since $\dot{\alpha} : \dot{\theta} :: \varrho : 1$) $z' \varrho\,\dot{\varrho}\,\dot{\theta} = \sqrt{r^2 - \varrho^2}\,\varrho\,\dot{\varrho}\,\dot{\theta}$; whence $s = \int\int \sqrt{r^2 - \varrho^2}\,\varrho\,\dot{\varrho}\,\dot{\theta}$, as before.

2. The intersection of the surfaces of the cylinder and sphere is a curve of double curvature, of which the two equations are $y = (r x - x^2)^{\frac{1}{2}}$ and $z = (r^2 - r x)^{\frac{1}{2}}$; and hence it appears, that the same curve may likewise be formed by the intersection of the surfaces of two cylinders, one perpendicular to the plane of x and y on a circular base, the other perpendicular to the plane of x and z on a parabolic base.

3. The area of the curve of double curvature $= \int z' (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}}$ $= \int (r^2 - x^2 - y^2)^{\frac{1}{2}} (\dot{x}^2 + \dot{y}^2)^{\frac{1}{2}} = \int \frac{r^{\frac{3}{2}} \dot{x}}{2 \sqrt{x}} = r^{\frac{3}{2}} x^{\frac{1}{2}}$, when $x = r$, $= r^2$; this area is the surface of half the cylinder included in the hemisphere, and therefore the surface of the two cylinders included in the sphere $= 8\,r^2$.

* The method of finding triple and double integrals, is not confined to the solution of merely geometrical problems like the present. The attraction of an elliptical spheroid depends on the formula $\int\int M \dot{x} \dot{y}$; and, by integrating it, M. LAGRANGE (Mem. de Berlin,) and M. LEGENDRE (Mem. de l'Acad. 1788), gave the analytical solution of the problem of the attraction of a spheroid, which MACLAURIN had solved, in his Treatise of the Tides, on purely geometrical principles.

4. The length of the curve cannot be algebraically expressed; but it may be exhibited by means of the rectification of an elliptic arc; for * $\int \sqrt{(\dot{z}'^2 + \dot{y}^2 + \dot{x}^2)} = \int \dot{x} \sqrt{\left(1 + \frac{(r-2x)^2}{4(rx-x^2)} + \frac{r^2}{4(r^2-rx)}\right)}$ $= \frac{\sqrt{r}}{2} \int \dot{x} \sqrt{\left(\frac{r+x}{x(r-x)}\right)}$.

If c and t are semiaxes of an ellipse, v abscissa measured from vertex, $\dot{\overline{\text{arc}}}\ (\dot{A}) = \dot{v} \sqrt{\left(\frac{c^2}{2tv - v^2} + \frac{t^2 - c^2}{t^2}\right)}$, let $v = t - mt\sqrt{x}$, then $\dot{A} = -\frac{mt\dot{x}}{2\sqrt{x}} \sqrt{\left(\frac{t^2 + m^2(c^2 - t^2)x}{t^2 - m^2 t^2 x}\right)} \therefore \int \frac{\dot{x}}{\sqrt{x}} \sqrt{\left(\frac{t^2 + m^2(c^2 - t^2)x}{t^2 - m^2 t^2 x}\right)}$ $= C - \frac{2A}{mt}$ (semiaxes t, and c, abs. $t - mt\sqrt{x}$). Compare this form with $\frac{\dot{x}}{\sqrt{x}} \sqrt{\left(\frac{r+x}{r-x}\right)}$ and $t^2 = r$, $m^2 = \frac{1}{r}$, $c^2 = 2r \therefore \frac{\sqrt{r}}{2} \int \frac{\dot{x}}{\sqrt{x}} \sqrt{\left(\frac{r+x}{r-x}\right)}$ $= -\sqrt{r} \times A$ (semiaxes $\sqrt{2r}$, $\sqrt{r}$, abs. $\sqrt{r} - \sqrt{x}$) + corr. (C) when $x = 0$, length of curve $= 0 \therefore C = \sqrt{r} \times$ quad. ellipse (Q). Hence, length $= \sqrt{r}(Q - A)$ when $x = r = \sqrt{r} \times Q$ $= Q'$ (Q' being quadrant ellipse of which the semiaxes are r, $r\sqrt{2}$); hence, if an ellipse be described, of which the semiaxes are the radius (r) of a great circle of the sphere, and the side of a square inscribed in that great circle, then the length of the curve line which is the intersection of the cylinder with the surface of the hemisphere, is equal half the periphery of the ellipse.

* The length of the curve may be as commodiously expressed in terms of ϱ; for, since $x = \varrho \cos.\theta$, $y = \varrho \sin.\theta$, $z' = \sqrt{r^2 - \varrho^2}$ $\sqrt{\dot{x}^2 + \dot{y}^2 + \dot{z}'^2} = \sqrt{\frac{r^2\dot{\varrho}^2 + (r^2 - \varrho^2)\varrho^2\dot{\theta}^2}{\sqrt{r^2 - \varrho^2}}}$; but $\dot{\theta}^2 = \frac{\dot{\varrho}^2}{r^2 - \varrho^2}$, therefore $\sqrt{\dot{x}^2 + \dot{y}^2 + \dot{z}'^2} = \dot{\varrho} \sqrt{\frac{r^2 + \varrho^2}{r^2 - \varrho^2}}$; whence the integral, by means of the rectification of an ellipse, as before.

VIII. *Account of the Discovery of Silver in Herland Copper Mine. By the Rev.* Malachy Hitchins. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read February 12, 1801.

HERLAND Mine is situated in the parish of Gwinear, about seven miles N. E. of St. Michael's Mount, on the southern coast of Cornwall; and two miles and a half from the mouth of the river Hayle, on the northern coast of the same county: it is contiguous to Prince George Mine.

It commences in a valley on the west, and passes through a hill, which is first of steep and then of moderate ascent, for upwards of half a mile eastward; when the principal copper lodes, which follow this direction, meet with a large cross lode, by which, and by other cross courses and flookans, which intersect them in their farther progress, they are repeatedly heaved, and so disordered by these heaves, in their form and position, and so changed by them, in respect to their composition, as hardly to be recognized.

The strata of the district in which this takes place, consist of the common metalliferous sort of argillaceous slate called *Killas.*

The copper lodes of this district are remarkable for the shortness of their continuity; for, whereas other lodes may be traced to an indefinite extent in the same line of direction, these, on the contrary, are observed to taper away gradually, and

terminate, to all appearance, at a short distance, completely and irrecoverably.

This mine was worked about twenty years ago, when it was sunk to the depth of one hundred fathoms from the surface. It was again set to work about eight years since; has now four fire-engines and two steam-whims on it; and is sunk to a depth of one hundred and fifty-five fathoms below the surface, or, as the miners call it, from *grass*.

It is in this latter period of its history, that a discovery has been made of a considerable quantity of silver ore, in a particular part of the mine, the singularity of which discovery, in this country, has much excited the curiosity of the public.

For, although the numerous veins of lead in Cornwall are richly impregnated with silver, and occasionally yield small quantities of silver ores, and even specimens of native silver, yet, hitherto, no instance had been known of their yielding this precious metal in such abundance; nor had any circumstances, in the natural history of the mineral veins of this country, borne any analogy to those which accompanied the present discovery.

These circumstances therefore, having been examined with more attention than usual, shall be stated with as much precision as it is possible to obtain, from the report of those practical miners only who have hitherto inspected them.

The facts which deserve to be first noticed are, the confined and insulated position of the mass of silver ore; its great depth from the surface of the mine; and its contiguity to a copper lode.

The lode in which it occurs is one of those cross courses, as they are here called, which intersect and derange the copper lodes, and consequently are of a more recent formation.

Lodes in this direction are usually filled with quartz, but frequently produce galena; and sometimes, instead of galena, sulphurated antimony. They appear here to conform to the same laws, except in the particular instance now to be described, which forms, indeed, a very remarkable exception.

No ores of silver were observable in this lode, until at the depth of one hundred and ten fathoms from the surface, or eighty below the adit or level; and, at the farther depth of thirty-two fathoms, they disappeared.

They have been discovered only in the neighbourhood of one of the intersected copper lodes, extending no where above twelve feet from this lode, on the north, or above thirty-two feet from it, on the south, and acquiring this their greatest extent at the deepest level; for, the usual dimensions of the silver ore are not more than six feet in the former situation, and twelve feet in the latter.

It is remarkable, that at the point of contact or intersection, the contents of the silver lode are so poor as to be scarcely worth saving; and those of the copper lode are much less productive of copper than at a little distance from this point. Moreover, that the copper lode, in the vicinity of the intersection, seems to have been influenced by the same causes of improvement and declension as the cross lode; being richer or poorer in copper, as the latter was, at a correspondent level, in silver.

The richest mass of silver ore was found at the depth of two fathoms above the level at which it disappears.

After this brief account of the most striking facts, it may be proper to enter into a more particular description of the two

lodes which appear, by their intersection, to have generated this body of extraneous matter.

The copper lode bears nearly east and west by the compass; the cross lode nearly north and south, or at right angles to it.

The former is about two feet broad, on an average; and it dips or underlies south, one foot in a fathom. The breadth of the latter is about two feet and a half, on an average; and its underlie is east, about eight inches in a fathom.

The heave of the copper lode is about eighteen or twenty inches to the right, in the language of the Cornish miner; the expression being so far appropriate and convenient, as it refers to the usual situation of the observer in the heaved lode.

The copper lode is filled with layers of ore and stony matter, the latter of which is here called *Caple;* but the ore is usually found contiguous to the walls of the lode.

The contents of the cross lode are more singular, in respect to their local position, and more various. Only the eastern side of it produces silver ore, the breadth of which is in general about six or eight inches, although in some places it is greater. The other part of the lode is chiefly composed of quartz, intermixed with iron, manganese, and wolfram, together with a small portion of cobalt and antimony.

The silver ore, strictly speaking, is a mixture of galena, native bismuth, grey cobalt ore, vitreous silver ore, and native silver; which, in respect to their proportions, follow the order in which they are here enumerated, the galena being the most prevalent. The native silver, of which specimens of the greatest beauty have been reserved for the cabinets of the curious, is found chiefly in a capillary form, in the natural cavities of the lode.

About one hundred and eight tons of this ore have been raised. The miners continue to sink near the same point of intersection; and seem confident that both lodes will soon become richer, because similar instances of declension and recovery have frequently occurred in the copper lodes of this mine, and because the two lodes appear to have a reciprocal influence on each other.

Unfortunately, however, the extent of their speculation is limited by the great depth of the present workings; for, forty-five fathoms have been sunk since the first discovery of the silver; and twenty, or twenty-five fathoms more, are as much as can be sunk in this mine, with its present mechanical powers of drawing the water; at which level, viz. one hundred and eighty fathoms from the surface, it would be somewhat deeper than any mine in Cornwall, and about one hundred and thirty fathoms below the level of the sea, at low water mark.

The other cross lodes in this mine produce no silver; most of them being flookans, or lodes which are essentially different from the argentiferous cross lode, in the nature of their constituent mass. There is one, however, in the eastern part of the mine, which, from its resemblance to that, is thought likely to produce silver, whenever it shall be explored to the same depth, at its point of intersection; although these hopes may probably be fallacious, for the argentiferous lode intersects five other copper lodes, viz. two on the north, and three on the south side, without producing any silver.

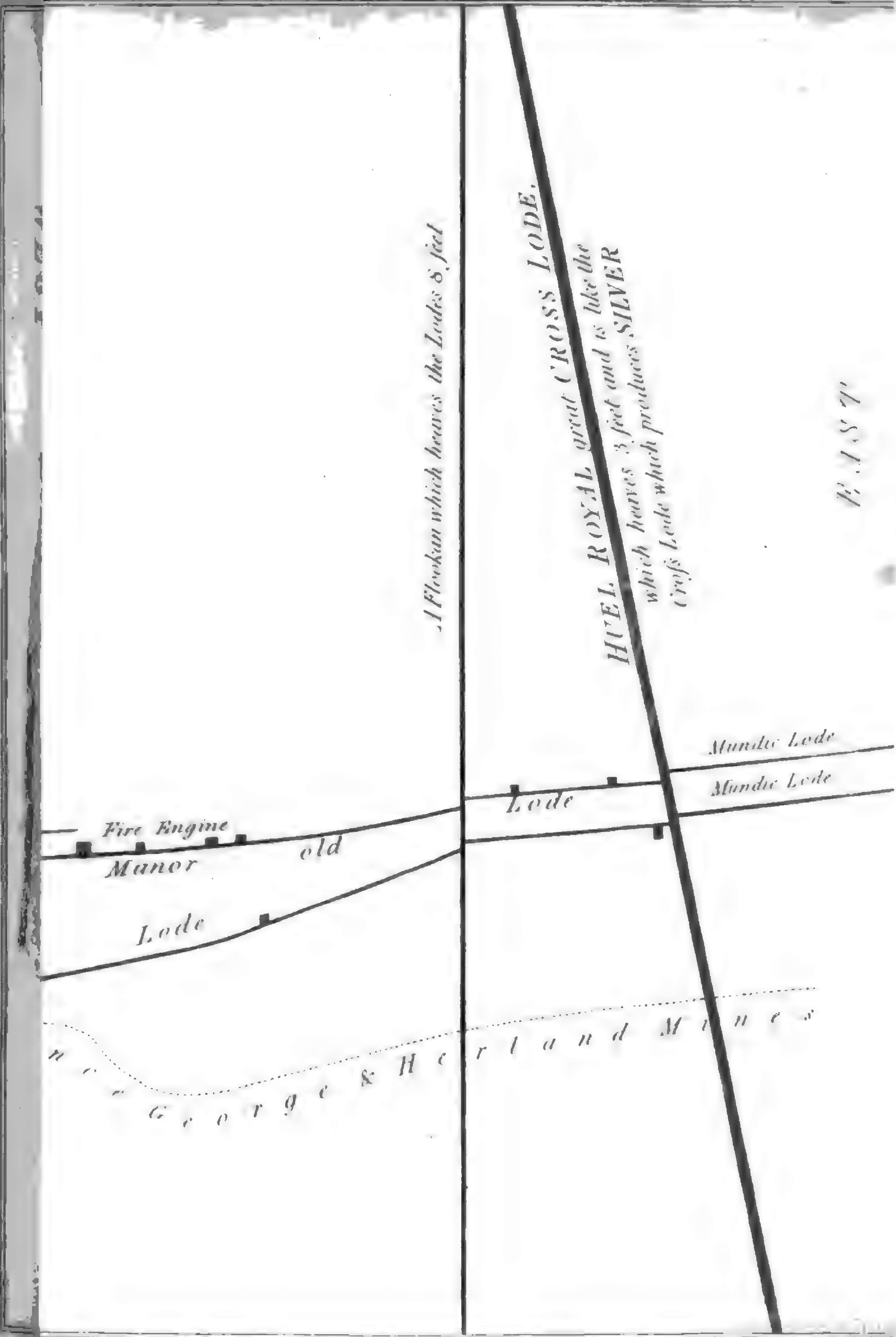
EXPLANATION OF THE PLATE.

Plate XI. represents the lodes of Herland and Prince George copper mines, on a plane parallel to the horizon, at one hundred and ten fathoms below the surface of the earth.

This plan is drawn on a scale of one inch to seventy-two yards.

The size of the lodes, and the distance of their heaves, are, for the sake of distinctness, represented too large in proportion.

The small black squares represent the shafts.


A Flookan which heaves the Lodes 8 feet
HUEL ROYAL great CROSS LODE,
which heaves 3 feet and is like the
Cross Lode which produces SILVER
EAST
Mundic Lode
Mundic Lode
Lode
Fire Engine
Manor
old
Lode
George & Herland Mines

IX. *Account of an Elephant's Tusk, in which the Iron Head of a Spear was found imbedded. By Mr.* Charles Combe, *of Exeter College, Oxford. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read February 19, 1801.

Sir,

I have the honour of transmitting to you a fact relative to an elephant's tusk, in which the iron head of a spear was lately discovered to have been imbedded.

The tusk weighed fifty pounds: it measured six feet in length; and was supposed, by Mr. Pope, an eminent manufacturer at Birmingham, to have come from Africa, as he procured it at a sale in Liverpool.

When it was delivered into the hands of the workmen, they perceived, on the tusk being shaken, a rattling noise, about two feet and a half from the base; and, in consequence, made a transverse section, somewhat below the part whence the sound proceeded. Here, upon enlarging the aperture by a chissel, they distinguished a hard extraneous body; and, on making other sections, found it to be an iron spear-head, considerably corroded.

It is no very uncommon circumstance to meet with brass, lead, and iron musket-balls in the substance of an elephant's tusk; but I believe a spear-head, in a similar situation, has not hitherto been observed. Besides, general appearances seem to indicate, that balls are projected through the sides of the tusk;

whereas, in the instance before us, it is hardly possible that the accident could have taken place in that way. The texture of the surrounding ivory bears no marks of external injury; and the spear-head pursues the natural course of the cavity, pointing downwards towards the apex of the tusk.

The most probable conjecture is, that the spear entered at the basis of the trunk. If we examine the skull of an elephant, it will be found, that the tusks are strongly articulated in the upper maxillary bones. In the males, they reach as high as the thin plate, which separates them from the nasal cavity, whence the trunk arises. We have only then to suppose, that the spear struck somewhat perpendicularly, between the interior angle of the eye and the proboscis: the interposing plate of bone would yield without much difficulty; and the cavity of the tusk is placed immediately beneath.

Whether the wooden part of the spear was separated directly, in consequence of the jar, or afterwards, by the exertions of the animal itself, is of little moment: no vestige of it now remains; and the head of the spear affords a presumption, that the shaft was never very firmly attached to it.

The presence of an extraneous body in the substance which fills the conical cavity of the tusk, would be the cause of inflammation, and subsequent suppuration. In the mean time, the spear-head, acting by its gravity, would descend, till prevented by the resistance of the converging parietes of the cavity. After a process of time, when the tusk had been protruded further from the skull, in consequence of growth, fresh bony matter would necessarily be deposited, to preserve a corresponding relation between the size of the cavity and the tusk; and thus the spear-head would gradually become imbedded within the ivory.

It is not, however, closely encompassed: there is a space, measuring about the third of an inch, on each side, between the face of the spear-head and the lateral limits of the cavity. Above the spear-head, the cavity very suddenly contracts in its long diameter, by an increased deposition of true ivory on one side: on the other, and in different places, we may perceive an attempt at subsequent bony formation; but it is imperfectly attached to the true ivory, and of an inferior quality, apparently consisting of a larger portion of animal, and a less of earthy particles.

It may be remarked, that there is a partial alteration of position, in that part of the cavity of the tusk which is occupied by the spear-head. Through this space, the long diameter runs in the direction of the short diameter of a transverse section of the tusk. There can be no doubt but that this alteration has arisen from the casual situation, which the spear-head first obtained. For, immediately above the spear-head, the natural position is resumed, and the long diameter of the hollow is with the long diameter of the tusk.

Were we acquainted with the rate of progress which a tusk assumes in growth, we might make some estimate of the age of the elephant, when the accident took place. There are, however, I believe, no *data* from which any correctness in this respect can be collected. The elephant certainly recovered; and, from the situation of the spear-head, together with the quantity of bony matter afterwards deposited, it is probable that the animal lived a considerable time after the wound had been received.

I am, &c.

CHARLES COMBE.

EXPLANATION OF THE FIGURES. SEE PLATE XII.

The description of the drawings (which are upon a scale of half an inch to an inch,) supposes the natural situation of the tusk, with its apex pointing downwards.

Fig. 1. Shews the shape and size of the spear-head.

Fig. 2. Position of the spear-head, in a transverse section of the tusk, with the relative magnitudes of both.

Fig. 3. Cavity of the tusk below the spear-head, after it had been enlarged by the chissel.

Fig. 4. Cavity, as it surrounds the spear-head, with its long diameter running in the direction of the short diameter of the tusk. *a.* A portion of more recently formed inferior bony matter.

Fig. 5. Cavity of the tusk just above the spear-head. The portion faintly shadowed represents the contraction of the cavity by true ivory. *a a.* More recently formed inferior bony matter.

Fig. 6. The natural position of the long diameter of the cavity resumed, so as to run in the direction of the long diameter of the tusk. *a a.* More recently formed inferior bony matter.

Fig. 3, 4, 5, and 6, Represent the lower surfaces of the connecting transverse sections of the tusk. Fig. 3, is farthest from the basis; and their respective lengths are, Fig. 3, 4, and 5, 3½ inches each; Fig. 6, 2 feet.

Fig. 2.

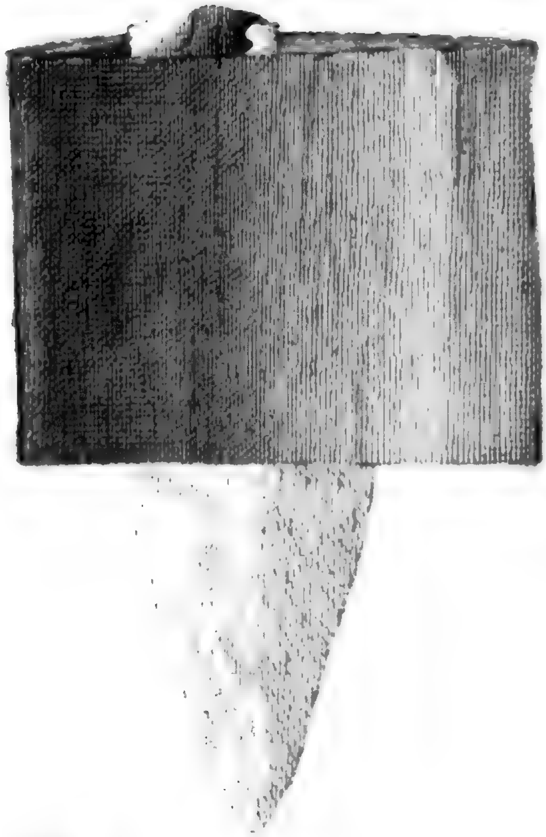


Fig. 4.

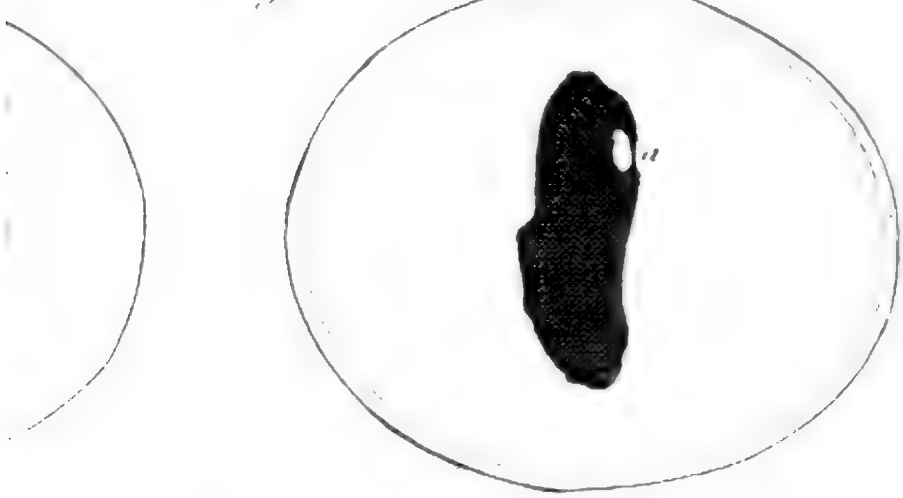
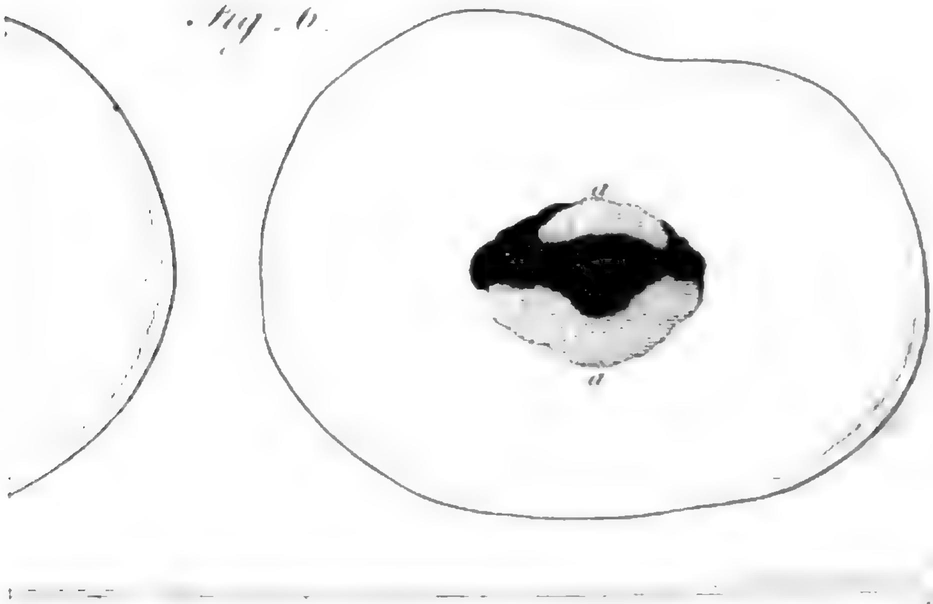


Fig. 6.



X. *Description of the Arseniates of Copper, and of Iron, from the County of Cornwall. By the* Count de Bournon. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read February 19, 1801.

SECTION I.

ARSENIATES OF COPPER.

The natural combination of the arsenic acid with copper, and the different aspects under which this combination appears, according to the proportions in which these two substances are united, were among those objects of mineralogy, respecting which our imperfect knowledge required the aid of study and observation. A new copper mine, lately worked, called Huel Gorland, in the parish of Gwennap, in the county of Cornwall, having, within the last two years, enriched the cabinets of London with some very fine specimens of these arseniates, I have been induced to pay particular attention to them; and I offer the result of my observations to the Royal Society, as an acknowledgment of that gratitude which I and all Frenchmen, faithful to their king, ought to feel and profess to a country which has distinguished itself as the protector of honour and loyalty.

Although there appears, according to some German authors, reason to think that arseniate of copper has been found in Silesia, its much greater abundance, as well as the various

aspects under which it exists, in the county of Cornwall, may entitle it to be considered as one of the many mineral substances which are peculiar, or nearly so, to England.

Of the various works on mineralogy lately published, there are few which have not mentioned arseniate of copper, among the ores of this metal. It seems, however, that some of their authors had no knowledge of this ore, except from the very imperfect account communicated by the celebrated KLAPROTH, in 1787, in the *Schriften der Gesellschaft Naturforschender Freunde*, Vol. VII. in which he has given an interesting sketch of the mineralogy of the county of Cornwall, as far as it was then known. Others seem to have possessed only imperfect specimens of arseniate of copper, as none of the forms which they attribute to its crystals, can belong to it. Besides, they all confound with this ore, those cubic crystals, of a very beautiful green colour, which are found in Muttrell mine, contiguous to Huel Gorland mine; and which, according to the analysis made with the greatest care and ability, by Mr. CHENEVIX, are of a nature totally different, and cannot properly be classed among copper ores, as they contain but a very inconsiderable quantity of that metal.

The existence of arseniate of copper seems, however, even at this day, to be an object of doubt among the French mineralogists; for the Abbé HAUY does not mention it in the 28th and following numbers of the *Journal des Mines*, although they contain an interesting extract of a system of mineralogy, which he was then preparing for the press; nor has M. FOURCROY even hinted at it, in his *Système des Connoissances Chimiques*, lately published.

It is now above twenty years, since arseniate of copper was

discovered in the county of Cornwall; it was first found either in Carrarach mine, in the parish of Gwennap, or in Tincroft mine, in the parish of Allogan. Its matrix, like that of almost all the copper ores of this country, was siliceous, and consisted of a decomposed granite, of which the greatest part of the feldspar had passed into the state known by the name of *Kaolin*. It was accompanied with grey vitreous copper ore, frequently in considerable masses; also with much black oxide of copper; and with various oxides of iron.

The arseniate here spoken of, which had never been found in large quantity, had ceased to exist in the above-mentioned mines, when Huel Gorland mine, lately wrought, began to enrich mineralogy with this uncommon substance. The matrix of this is likewise siliceous; sometimes crystalline; and sometimes in an amorphous mass. Here and there we find mixed with it, in greater or less profusion, all the known oxides of copper; many of the argillaceous oxides of iron; also grey vitreous copper ore; arsenical pyrites; and the rich deep-coloured yellow copper ore. This last is often found differing from its usual appearance, in a manner which I believe has not hitherto been taken notice of. I think it should make a distinct variety among the deep yellow copper ores, under the name of *yellow hematitic copper ore*.

When the combination of copper with iron and sulphur is rich in metal, (for when it is poor, it is only a martial pyrites mixed with a little copper,) its appropriate colour, when a piece is fresh broken, is a deep yellow; and this yellow colour is more deep, in proportion as the quantity of copper is more abundant. In its richest state, it assumes a more or less greenish tint. The surface of a fresh fracture is very brilliant, and appears rather

uneven, as if composed of small laminæ, crossing one another in an irregular manner. When it begins to decompose, its surface is covered with the most beautiful colours; among which the most predominant are, violet, blue, and green: this has caused it to be compared to a pigeon's breast. When these colours are very deep, and occupy the whole surface of a piece, we commonly perceive here and there, some little points, in the state of red oxide of iron; and others of a green colour, in the state of green carbonate of copper.

This kind of copper ore is sometimes of a deep yellow colour, which inclines the more to green, as it is destitute of brilliancy. It is very compact, and, when broken, the fracture appears smooth, sometimes a little conchoidal; its surface, however, has a very fine grain, which, when viewed with a powerful lens, resembles the aggregation of a very close compact mass of the finest sand. Its most usual texture is that of thin layers or plates, lying one upon another, and being very closely united, so as to be scarcely perceptible to the naked eye; but they are very readily distinguished with the lens. These layers, however, do not adhere very strongly, as they may always be separated by the stroke of a hammer.

This ore frequently assumes a mamillated form; the mamillæ being of different dimensions, from the size of a man's head, and even larger, to that of a small pea. In the latter case, the mamillæ are very often united, as in that kind of iron ore which is called cluster, botryoid, or kidney hematites. Sometimes the surface of the mamillæ is covered with small points; but more frequently it is smooth, so as very much to resemble a piece of polished metal; and, as the surface of the mamillæ inclines rather to a brown colour, they have the appearance of

antique bronze. The green oxide of copper which sometimes is observed on it, completes the illusion, by assuming the aspect of that fine patina which often covers pieces of antique bronze.

This ore is likewise found in the form of small cylinders, often placed one against the other, and sometimes ramified, in the same way as is observed in some kinds of hematites. When the broken surface of it is exposed for some time to the air, it assumes the colour of tarnished gold. It acquires also, by the decomposition of its surface, the same violet, blue, and green colours as the kind already described; but, although these colours are frequently very deep, they never are so brilliant as in that kind.

It is very uncommon to find pieces of this ore that are not mixed, and frequently even penetrated, with grey vitreous copper ore. That which accompanies the arseniate of Huel Gorland mine, offers, in this respect, an uncommon and very particular appearance. The yellow ore is mechanically mixed with the vitreous ore, so as to form a compound, in which, by the assistance of a lens, the small particles belonging to each of those two ores, may be very clearly distinguished. The specific gravity, as well as the quantity of copper, in this ore, varies considerably, according to the proportions in which the yellow copper ore and grey vitreous copper ore are mixed together; sometimes they seem to be mixed in equal proportions, or nearly so.

Nature has established very remarkable differences between the arseniates of copper; and these take place not only in their forms, but likewise in their hardness and specific gravity. These differences arise, either from the manner in which the arsenic acid is combined with the copper, or from the different proportions

in which these two substances are combined. I have been naturally led to follow the same order, and to divide the arseniates of copper into four different species: and the very interesting analysis of this substance, made by Mr. CHENEVIX, has afforded me the most satisfactory sanction to this division. It is thus that the chemist and the naturalist, by freely uniting their labours, without jealousy or prejudice, ought in all cases to proceed, in order to attain that certainty which is the desirable recompense of their endeavours.

FIRST SPECIES. *Arseniate of Copper in the form of an obtuse octaedron.*

The most simple form under which this species appears, is a very obtuse octaedron, formed by the united bases of two tetraedral pyramids, with isosceles triangular planes; and this appears to be its original form. This octaedron has, in each of its pyramids, two opposite planes more inclined than the other two; which gives a parallelogrammic form to their common base. (Fig. 1. Plate XIII.) The two planes more inclined than the others, meet at the apex of each of the pyramids, in an angle of 130°; and at the common base, in one of 50°. The two planes which are less inclined, meet at the apex, in an angle of 115°; and at the base, in one of 65°.

These planes are commonly smooth and bright; sometimes, however, they are striated, in a direction parallel to their edges.

The four planes seldom terminate in one and the same point: more commonly the apex is formed into a ridge, the octaedron being lengthened, parallel to its less inclined planes; the base is then a square, or at least approaches very nearly to that form. (Fig. 2.)

These two varieties are the only ones I have observed in the form of the crystals of this species, although I have had the opportunity of examining a great number of specimens.

This arseniate is very light; its average specific gravity, taken on six pieces perfectly pure, was 2,881. Its hardness is likewise very inconsiderable; it easily scratches calcareous spar, but makes no impression on fluor spar.

It is seldom perfectly transparent; but has generally a cloudy aspect.

The usual colour of this species (for this character is as essential in metallic substances as it is immaterial in stones) is a beautiful deep sky-blue; sometimes, but very rarely, it inclines more or less to Prussian blue. It is frequently of a very fine grass-green; the crystals have then a much more beautiful transparency. I have seen some which were of a fine apple-green; others white, having a slight blue cast. In one piece, the crystals of which were of a green colour, and less transparent than they commonly are, I discovered, on breaking them, that the colour of their central part, for about half their thickness, was blue. From the observations made by Mr. CHENEVIX, in his analysis of these arseniates, it appears that the variation in their colour, principally depends on the quantity of water, which enters as a constituent part into their formation.

This species is found mixed with all the other kinds of arsenical copper ore; but that which most commonly accompanies it, is the prismatic triedral species.

I have never discovered in this species, any thing which could induce me to suppose it susceptible of decomposition, or even of change.

SECOND SPECIES. *Arseniate of Copper in hexaedral laminæ, with inclined sides.*

This species is commonly found in very fine hexaedral laminæ. The six narrow sides of these laminæ have an inclined position, alternately in a contrary direction, on the two broad planes, in such a manner that each of the planes is encompassed by three sides, which are inclined upon it. (Fig. 3.) As far as the small size, and more particularly the thinness of these crystals, has enabled me to judge, two of these three inclined sides, form an angle of about 135° with the broad planes on which they incline; and the third, one of 115°.

The two broad planes are smooth, and have a very brilliant lustre. The six narrow sides are rendered very dull, by the great number of striæ with which they are covered; most of which are very prominent, and all are parallel to the edges of the broad planes. In consequence of this, these crystals may be divided, parallel to the planes, almost as easily as crystals of mica.

This structure prevents the crystal from being considered as a modification of the octaedron: that which would be produced by an increase of the inclined sides, would only be a secondary crystal; and none of the specimens I have seen, give mereason to suppose the existence of such a variety.

The colour of this species is a fine deep emerald green; sometimes, though rarely, it is found of a lighter colour. The lustre of its broad planes, which are the only parts of the crystal that forcibly strike the eye, give it pretty much the appearance of those coloured metal plates which are known by the name of foil.

This species is still less heavy than the preceding; its specific gravity being only 2,548.

It is also less hard; it easily scratches gypsum, but not calcareous spar.

When its crystals are very thin, they are very transparent; but their transparency is diminished when they have any degree of thickness.

When exposed to fire, this species decrepitates very strongly.

This arseniate, the matrix of which is generally quartzous, is occasionally found mixed with some other arseniates of copper, and particularly with the acute octaedron in the capillary or fibrous state. (Spec. 3. Var. 1 and 2.) But the ore most commonly found with it, is the red copper ore, which is frequently very abundant.

I have never observed in this species any appearance of decomposition.

THIRD SPECIES. *Arseniate of Copper in the form of an acute octaedron.*

The most simple form in which this third species is found, is likewise an octaedron; but this octaedron, instead of being obtuse, like that of the first species, is slightly acute. It has, like that, in each of its pyramids, two opposite planes more inclined than the other two. The more inclined planes meet at the apex, in an angle of 84°; and at the base, in one of 96°: the other two meet at the apex, in an angle of 68°; and at the base, in one of 112°. (Fig. 4.)

In this octaedron it sometimes happens, that the planes which compose its pyramids tend to one and the same point, in order to form the apex; but it is much more common to find it extended in a line parallel to the less inclined planes of the

pyramid. (Fig. 5.) The crystal is still more frequently found in the form of a long tetraedral rhomboidal prism, of 84° and 96°, terminated by a diedral apex, with isosceles triangular planes, which are placed on the angles of 84°, and meet in an angle of 112°. (Fig. 6.)

Most commonly, both in the perfect and the lengthened octaedron, the angles of 96° are replaced by a plane, which is equally inclined on the adjacent sides, (Fig. 7.) and is frequently very broad: (Fig. 8.) then the tetraedral prism of 84° and 96°, is changed into a flat hexaedral prism, having two angles of 84°, and the other four of 138°. I never saw the angles of 84° replaced.

The average specific gravity of this arseniate of copper, taken on five pure pieces, was 4,280.

It is sufficiently hard to scratch fluor spar; but is not hard enough to scratch glass.

Its usual colour is a brown, or bottle green, so dark that the crystals appear of a blackish colour, when they are not opposed to the light; sometimes, but very seldom, in the regular crystals which happen to be rather thicker, this colour is a clearer green; in other specimens, the crystals have a yellowish cast, and the surface then often reflects the light of a golden tint.

The transparency of this species is generally pretty great.

It is not always crystallized in a determinate form, but is an absolute Proteus, both with respect to the different forms in which it appears, and the various colours it exhibits. I have observed the five following varieties of it.

Variety 1. *Capillary, of a determinate form.*

In this variety, the crystals are extremely slender, yet preserve their form, which is that of a very lengthened octaedron. The small slender crystals often form themselves into a confused group; sometimes, however, they form small mamillæ, by the divergence of a number of them from a common centre. Their colour is either a fine grass green, or a yellowish green, or a golden yellow; and they have generally a beautiful transparency.

Variety 2. *Capillary, of an indeterminate form.*

In this variety, the very thin needle-like crystals are not terminated by the diedral apex of 112°, representing two planes of the octaedron, but gradually become smaller, and terminate in a very sharp pyramid. This variety has the same colours as the preceding; and its very slender crystals are grouped in the same manner as in that.

Substances in a crystallized state, in passing from a determinate form to an indeterminate or fibrous one, frequently assume an intermediate form, in which the crystal insensibly terminates in a very acute pyramid.

Variety 3. *In crystals perfectly regular for a part of their length, and fibrous at their extremity.*

In this variety, the crystals are perfect during a part of their length; but their substance insensibly divides as it approaches the extremity, which very often is in fact nothing but a cluster of extremely delicate fibres, the colour of which always appears lighter than that of the solid part of the crystal.

Variety 4. *Amianthiform.*

This variety is composed of fibres as delicate as those of amianthus, of the flexibility of which they frequently possess a certain degree. These fibres are either parallel, or divergent from one common centre, in which case they nearly resemble a hair pencil. Their colour varies considerably: I have seen them of different shades of green, from a grass green to a dark brown green, of a golden brown, of a straw colour, of a golden yellow, of a greenish blue colour, and even perfectly white, having frequently the lustre of satin.

The fibres are sometimes so delicate, so short, and so confusedly grouped together, that the whole appears like a dusty cottony mass, the true nature of which is discoverable only by the lens. At other times, this variety appears in small thin laminæ, rather flexible, sometimes scarcely perceptible to the naked eye, sometimes tolerably large, and perfectly like *amianthus papyraceus*. I have seen the last mentioned form of this variety, of a light green colour, and also of a very delicate white.

Variety 5. *Hematitiform.*

This variety is in layers, either flat or mamillated; and is of a fibrous texture; but is rendered compact by the close manner in which the fibres are united to each other, in the same way as is observed in many martial hematites, and more particularly in that kind of tin ore which is known by the name of wood-tin, to which, some pieces of this arseniate of copper have a very great resemblance. Yet it sometimes happens, as in many aggregate pyrites of a globular form, that the surface of the

small mamillæ is covered with little rough points: these are the diedral apices, which terminate the little crystals supposed to contribute to their formation.

This hematitic variety is found with the same diversity of colours as the preceding, or amianthiform variety.

FOURTH SPECIES. *Arseniate of Copper in the form of a triedral prism.*

The primitive form of this species is a triedral prism, the bases of which are equilateral triangles; (Fig. 9. Plate XIV.) this prism is often considerably lengthened, in a direction parallel to one of its bases. (Fig. 10.) This form is one of the most rare in crystallography. The crystals have all their sides smooth and brilliant; yet there are observable in some of them, when examined with a magnifying glass, transverse striæ on the sides of the prism, all of which are parallel to the edges of the bases. It is, therefore, chiefly on the planes of the bases, that the crystalline laminæ appear laid upon one another, to produce either the increase or the modification of the primitive crystal.

As the crystals of this species are seldom sufficiently detached to be easily perceived, and indeed are very frequently so small as to escape the observation of the naked eye, I think it necessary to describe here all the various forms in which I have seen them, with the progress observed in their passage from one form to the other, however small the difference between these forms may appear. Such a description will lead to a better knowledge, not only of the primitive crystal itself, but also of those forms of it which seem to be the most distant from its original one.

Very frequently, the triedral prism passes to a tetraedral modification, by the simple replacing of one of its edges by a plane, equally inclined on the adjacent ones. This plane is either very narrow, (Fig. 11.) or of a more considerable width. (Fig. 12.) Sometimes the width of the plane is such, that it reduces the primitive adjacent planes to extreme narrowness. (Fig. 13.) In this last case, the crystal appears under the form of a rectangular plate or lamina, having two of its narrow opposite sides or planes inclined, in one and the same direction, on one of the two broad planes. It sometimes happens, in this variety, that the two broad opposite planes approach more or less to a square form. (Fig. 14.) I have also seen some crystals, in which the two other edges of the prism seemed to have likewise very slight secondary planes; but, when that happens, they are always very narrow, especially when compared with the secondary plane of the third edge. This modification, in its various forms, is the most common one of this species.

Sometimes one of the solid angles of the triedral prism is replaced, on one side only, by a plane that is much inclined to the edge of the prism on which it is situated; but the crystals are always too small to admit of being measured with precision. (Fig. 15.) This plane, assuming a more considerable extent, replaces the same edge of the prism by another plane, much broader at one extremity than the other, as is shewn by the lines of large dots in the same Figure. Sometimes it has a very considerable extent, as is represented in Fig. 16. It then reduces one of the bases of the triedral prism to the form of a very narrow trapezium, while that of the opposite base remains very broad. By a still more considerable increase, the plane of this base totally disappears; and the crystal is termi-

nated, at that extremity, by a ridge. (Fig. 17.) In this variety, the crystal is often seen placed upon one of its scalene triangular sides, and then presents the other at its upper extremity; an appearance which is apt to puzzle the observer, particularly when he perceives, among the triangular sides which most generally terminate the crystals, nothing but equilateral triangles. This modification, in all its forms, is much less common than the preceding one; it is, however, occasionally met with.

The modification which we have just seen the primitive crystal assume at one of its solid angles, and only on one side, sometimes takes place also, (only on one side,) at its two other angles. Then, if the act of crystallization has continued so long, under the same mode of increase, that the new edges, as A B, (Fig. 15,) produced by the replacing of the solid angles, unite together, and give birth to a new equilateral triangle, placed in a direction contrary to the primitive one; and if, at the same time, the crystal has such a length that the secondary planes terminate at the opposite base, and are very acute isosceles triangles, the crystal will present the appearance of a kind of truncated hexaedral pyramid, the base and apex of which will be equilateral triangles. (Fig. 18.) The six triangular planes which compose the pyramid of this crystal, are always acute isosceles triangles; but three of them have their acute angle much smaller than the three others. The sides of the base of this kind of pyramid are opposite to the least acute angles; and its truncated apex is opposite to the most acute ones; the triangular planes being placed alternately in an opposite direction. I have seen several instances of this form; but I never saw such intermediate varieties as the

secondary plane, represented by the dotted lines in Fig. 15, would give, if it existed at the same time in the three angles.

By a longer duration of the act of crystallization, under the same modification, the plane corresponding to the truncated apex of the pyramid (Fig. 18.) becomes progressively smaller; the most acute isosceles triangular planes, which answer to the secondary ones, encroach on the least acute, all which are the primitive planes of the crystal, and the pyramid becomes truly triedral at its upper extremity, whilst it remains hexaedral at the base, on account of those parts of the three planes of the primitive crystal which are still preserved. (Fig. 19.)

By a still more considerable duration of the act of crystallization, the pyramid would become completely triedral, and would not be truncated at its apex I have never met with this modification so complete; but I have seen the variety represented in Fig. 19, which however, as well as Fig. 18, is very uncommon.

The triedral prism is subject to a fourth modification, which takes place at the three edges of one of its two bases or terminal surfaces only, and replaces each of those edges by a plane, much more inclined on the side of the prism on which it is placed, than on the terminal surface. (Fig. 20.) I have not been able to determine, in these crystals, the angles formed by these new planes, either with the sides of the prism, or with the terminal surfaces; but the varieties belonging to this modification, demonstrate that these angles are the same as those which the secondary planes of the solid angles make, either with the terminal surfaces, or with the edges of the prism on which

they are inclined. When these new planes have acquired an increase of sufficient extent to make the primitive planes of the prism totally disappear, and to replace them, the crystal is changed to a triedral pyramid with a truncated apex; the base and truncated apex of which are equilateral triangles. (Fig. 21.) When it happens that the crystal has, at the same time, gone through this modification and that which replaces the solid angles of its other extremity, and these two modifications have commenced at the very origin of the formation of the crystal, there is a particular period of its progress, in which the crystal is lengthened into a hexaedral prism, with acute triangular isosceles planes, having for their bases two equilateral triangular planes, perfectly equal. (Fig. 22.) After this period, if the act of crystallization continues, the crystal assumes the appearance of an extremely acute rhomboid, the acute solid angles of which are replaced, more or less completely, by an equilateral triangular plane; (Fig. 23,) and finishes at last by taking the form of a perfect rhomboid. (Fig. 24.)

All these varieties, though less common than those of the first modification, are yet frequently to be met with, excepting that of Fig. 22, which is extremely rare, and of which I have seen only two or three crystals; in general, however, the crystals of these varieties are very small, and their form cannot be well seen without the assistance of a magnifying glass.

It frequently happens, that two of the elongated triedral prisms (Fig. 10,) are closely united to each other, by one of the sides of the prism; whence results a kind of macle, (Fig. 25,) the form of which is a rhomboidal tetraedral prism, of 60° and 120°; but there is always discernible, on the terminal sur-

faces of these prisms, a very fine transverse line, A B, on the small diagonal of the rhomboidal plane of these surfaces; this line shews the place of union of the two crystals of which the macle consists.

Sometimes, the two component crystals of this kind of macle, belong to the triedral prism which has a secondary plane in the place of one of its edges; it then has the form, either of a hexaedral prism, that has four of its sides (two and two in opposition) broader than the others, (Fig. 26,) or of one that has only two opposite sides broader, (Fig 27,) or of a regular one, according to the width of the secondary planes: in all these forms, the line A B, indicating the place of union of the two crystals, is perceptible.

It is not very common, as I have already observed, to meet with specimens of this species, in which the crystals are sufficiently detached to let their form be distinctly seen. In general, the crystals are grouped together in great numbers, and seem to penetrate each other, so as to form mamillæ, more or less round; or they form a kind of indented cylinders, which have some resemblance to the trundle of a mill. In that case, the part of the crystals which appears at the surface of these aggregations, commonly belongs to one of the sides of their prism, either the broad or the narrow one. But, when these aggregations form either a kind of cylinders, or of mamillæ in clusters diverging like a fan, there may be seen, at the two edges of the cylinder, or at the summit of the clusters, the whole of the equilateral triangular terminal planes, or trapezia, of one part of the component crystals.

The specific gravity of this species of arseniate of copper, is nearly the same as that of the preceding one; I found it to be

4,280. Its hardness, however, is not so great; it is with difficulty that it can be made to scratch calcareous spar.

The crystals of this species, when they have not undergone any change, are transparent, and of a very beautiful bluish green colour, or deep verdigrise; but their surface easily becomes decomposed, and turns black; the crystals are then totally opaque. It is indeed very seldom, and only in cavities recently exposed, that crystals can be found which retain their transparency and colour. Yet, as the change they undergo commonly takes place only at the surface, rarely penetrating to any great depth, their original colour may easily be restored, merely by slightly scraping the surface with a sharp instrument.

The above is the only change I have had occasion to remark in this species.

Sometimes, but very rarely, this species is found in the form of small hair pencils, with very delicate fibres; and as, in the specimens in which I observed this variety, the little fibrous tuft had preserved its beautiful verdigrise colour, nothing could exceed the beauty of their appearance.

I have likewise observed this species in a mamillary form, with a compact texture; but this variety, like the preceding, is extremely rare.

The matrix of this arseniate of copper is the same as that of all the preceding species; and that species which is most frequently found with it, is the arseniate in obtuse octaedra. It is also frequently accompanied with that kind of ore which is known by the name of azure copper ore.

SECTION II.

ARSENIATES OF IRON.

Muttrell mine, which is immediately contiguous to Huel Gorland mine, in the county of Cornwall, has produced some specimens of arseniates of copper, exactly similar to those described in the former part of this Paper. But this mine is still more interesting to mineralogists, on account of a combination found therein, of arsenic acid with iron, and also a double combination of that acid with both iron and copper.

The first mentioned of these arseniates seems analogous to those crystals, or cubes, of a fine green colour, of which some specimens had already been found in Carrarach and Tincroft mines, and which Klaproth, in his Memoir upon the Mineralogy of Cornwall, considered as belonging to the arseniates of copper; but, according to the analysis made by Mr. Chenevix, with all the care which his extensive knowledge and extreme zeal for science would naturally lead him to employ, it appears to be a true arseniate of iron, containing only a small quantity of copper; and even that quantity seems to be merely an accidental mixture. As, in the specimens from the old mines of Tincroft and Carrarach, the greatest part of the crystals adhered to vitreous grey copper ore, it is possible that some particles of that ore remained attached to the crystals; or, as I have frequently found to be the case, that some such particles had penetrated into the crystals, and that Mr. Klaproth had been thereby deceived, by finding in the button left by the blow-pipe, a much greater proportion of copper than this ore

really contains. The natural decomposition of this arseniate, which produces an oxide of iron of a fine reddish yellow colour, strongly confirms the result of Mr. CHENEVIX's analysis.

GMELIN, in his Principles of Mineralogy, printed at Gottingen, in the year 1790, had already supposed that these crystals could not belong to the substance which, in mineralogical publications, had been called *arsenical copper ore.* He had, consequently separated them, leaving them, however, among the ores of copper, under the name of *würfel ertz.*

The double combination of the arsenic acid with iron and copper, although it had appeared to exist in the arseniate just spoken of, in the mines of Tincroft and Carrarach, had not excited the attention of mineralogists. It is however possible, that the transparency, the brilliancy, and the pale blue colour of its crystals, might occasion them to be mistaken for crystals of a stony nature. Besides, their smallness might easily cause them to escape the notice of common observation, particularly when they are not in pretty large groups.

The matrix of these two arseniates is exactly the same as that of the arseniates of copper; consisting, like that, of quartz, mixed with yellow, grey, and vitreous ores of copper, with oxides of iron, and with mispickel. The mines of Huel Gorland and Muttrell, although not situated in the district of the tin mines, have yet produced some specimens of tin, the crystals of which are covered with those of the arseniate here spoken of. Two specimens of this kind are in the collection of Sir JOHN ST. AUBYN.

SPECIES I. *Simple Arseniate of Iron.*

This species crystallizes in perfect cubes; (Fig. 28. Plate XV.) sometimes, though rarely, they are a little flattened; their sides are smooth and brilliant.

The only modification I have observed in this form is, that four of the eight solid angles of the cube are replaced by an equal number of equilateral triangular planes, situated in such a manner, that every one of the sides of the cube becomes an elongated hexagon, having two angles of 90° each, and four of 135°. (Fig. 29.) Crystals modified in this way are very scarce. I have never seen but one such specimen, which is in the collection of Sir JOHN ST. AUBYN. The crystals of it are pretty large, and very well defined.

The specific gravity of this species is 3,000. Its hardness is just sufficient to scratch calcareous spar. Its crystals, which are tolerably transparent, are of a dark green colour, with a brownish tinge; sometimes they are rather yellowish; and there exist some specimens of a brown yellow colour, like resin. I have never seen this species in any other state than that of perfect crystallization.

Sometimes indeed a decomposition takes place, which causes the crystals to pass into the state of a pulverulent oxide, of a fine reddish yellow colour. In this case, as the bulk of the crystals is considerably diminished, there is perceived, upon breaking them, a considerable number of small cavities in their substance. These cavities are analogous to those which appear in the crystals of the spathose ores of iron, when they have passed into a similar state of decomposition.

Species II. *Cupreous Arseniate of Iron.*

The crystals of this species are of uncommon brilliancy, and are perfectly transparent. Their form is a rhomboidal tetraedral prism, having two of its edges very obtuse, and the other two very acute: but, owing to the minute size of these crystals, I have not yet been able to determine the measure of their angles. The prism is terminated, at each of its extremities, by a tetraedral pyramid, which is pretty sharp; and its planes, which are scalene triangles, unite by pairs, forming elongated ridges, which join the acute edges of the prism: in the other direction, they unite, also by pairs, so as to form a ridge which is less elongated, and joins the obtuse edges. Very often, the obtuse edges of the prism are replaced by planes (of greater or less extent) equally inclined upon the adjacent ones. (Fig. 31.) Sometimes the acute edges are also replaced in the same manner, but always by planes of less extent. (Fig. 32.)

The above are the only varieties I have observed of this arseniate. Its crystals seldom occur singly, being generally grouped together, in a very irregular manner; sometimes, however, they are so united as to assume a mamillated form, having the pyramids of the crystals which compose the mamillæ all placed upon the surface thereof.

The specific gravity of this arseniate is 3,400.

Its hardness is rather greater than that of the simple arseniate of iron: it scratches calcareous spar with greater facility, but does not scratch fluor spar, or heavy spar.

Its colour is that of a very faint sky-blue; sometimes the blue colour is a little deeper. I have seen some crystals which had the same brown resin colour as the preceding species; but they are very rare.

Hitherto, I have never met with this species in any other form than that of a perfect crystal.

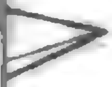
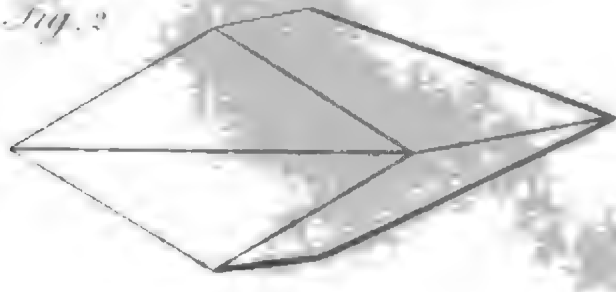


Fig. 2



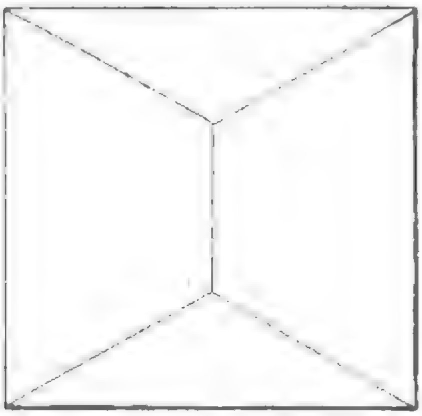


Fig. 4

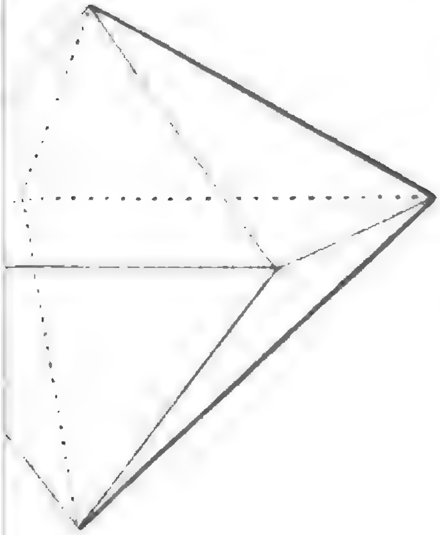


Fig. 5

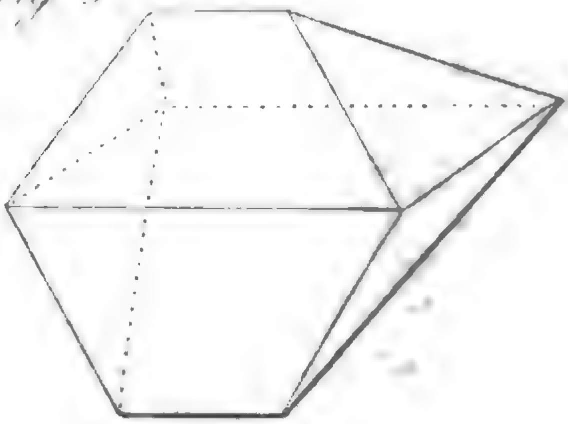


Fig. 7

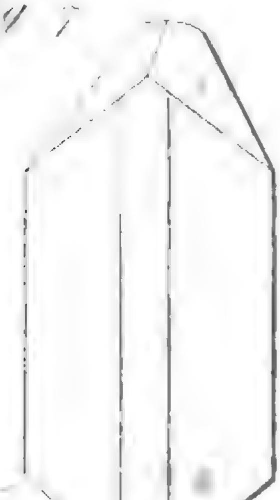


Fig. 8

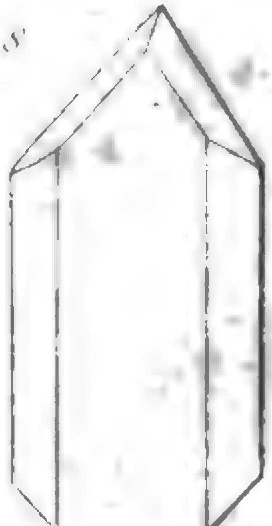


Fig. 11.

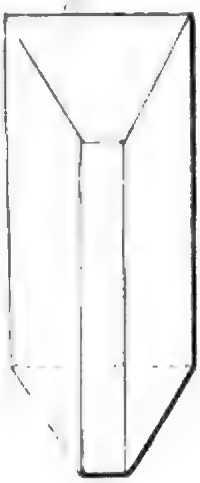


Fig. 12.

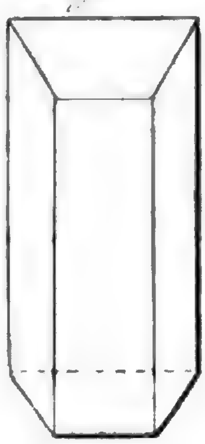


Fig. 13.

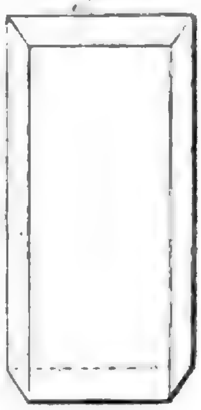


Fig. 16.

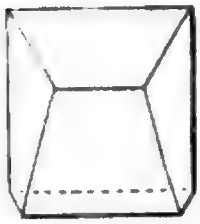


Fig. 17.

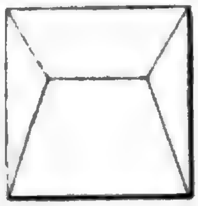


Fig. 18.

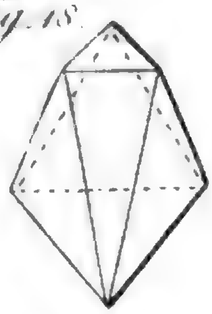


Fig. 21.

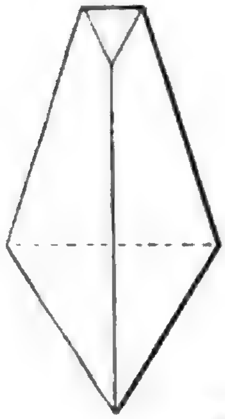


Fig. 22.

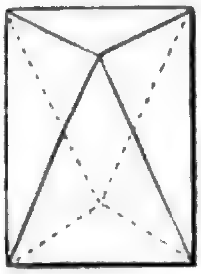


Fig. 23.

Fig. 25.

Fig. 26.

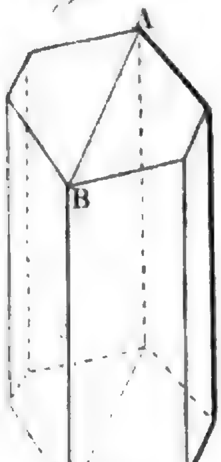


Fig. 27.

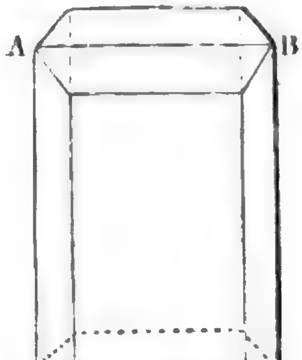


Fig. 29.

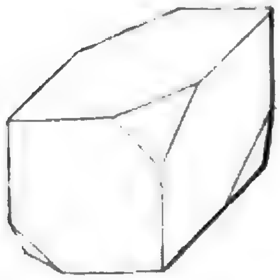
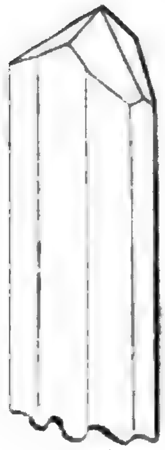


Fig. 31.

Fig. 32.



XI. *Analysis of the Arseniates of Copper, and of Iron, described in the preceding Paper; likewise an Analysis of the red octaedral Copper Ore of Cornwall; with Remarks on some particular Modes of Analysis.* *By* Richard Chenevix, *Esq. M. R. I. A. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read March 5, 1801.

SECTION I.

ARSENIATES OF COPPER.

THE endless diversity which the hand of Nature has diffused through all her works, even when she makes use of the same primitive materials, must sufficiently convince us, that, whatever accuracy we may attain in the knowledge of the latter, the means which she employs to form her combinations are still secret. The intellectual eye may indulge in the contemplation of hypothetic systems, which itself has created, and which it alone can behold; but, how far removed must they ever be from truths, evident to our senses, and supported by palpable experiments.

To follow Nature through the minutiæ of her labours, and behold her reproducing the same primitive materials in many different shapes, has always been deemed a less splendid achieve-

ment of science, than to discover one more of those simple substances, by the union of which she forms the complicated effects we daily admire. Yet to me it appears, that, in no instance is she more truly wonderful, than in the unbounded variety which she has sometimes produced from a small fund of original resources; and, when we can fairly follow a few primitive substances, through a series of combinations infinitely multiplied.

In addition to the two chymists who, as is mentioned in the preceding Paper by the Count de BOURNON, appear to have had some knowledge of the existence of a natural arseniate of copper, I must name M. VAUQUELIN. In a letter to me last year, he communicated the discovery of such a substance in France. Of the different varieties which these gentlemen, Messrs. KLAPROTH, PROUST, and VAUQUELIN, have examined, I shall have occasion to speak, in the course of these experiments: but it was reserved for the Count de BOURNON, to state, in the said Paper, with his usual talent and perspicuity, the scientific detail of the external characters, particularly of the crystalline forms, by which he had identified their nature. The free access to the extensive collections of the Right Hon. CHARLES GREVILLE, and of Sir JOHN ST. AUBYN, also the easy communication with the native soil of this mineral, were the peculiar advantages, which enabled the Count de BOURNON and myself to pursue the mineralogical and chymical researches, which are stated in these communications to the Society.

When the Count de BOURNON had completed what appeared to him to be the mineralogical classification of these copper ores, he gave me some specimens of each kind,

numbered indiscriminately, for the very purpose of excluding prejudice; and it was not till my task was ended, that we compared our observations. If I had been admitted into any previous knowledge of the arrangement dictated to him by the principles of crystallography, I should have been afraid, that I had merely thought true, what I wished to be so. But I can, most conscientiously, indulge in the satisfaction which the according results of different means to prove the same proposition naturally excite; and which is justly due to the truth of the outward marks, however delicate, yet still to be perceived, that nature has left visible to those who will observe her.

I shall now proceed to offer the result of a chymical analysis, undertaken with a view to determine what confidence the crystallographical arrangement, adopted in the preceding Paper, might merit; and to shew how far sciences so nearly allied, may receive new light and confirmation from reciprocal aid.

I shall confine myself to detail only those general processes which, upon frequent trial, have been found preferable. By reducing to powder any of the arseniates of copper here spoken of, and then exposing them to heat in a platina crucible, the water of crystallization was quickly dissipated. But, as too great a degree of heat volatilized some portion of the arsenic* acid, it was found necessary to moderate the heat;

* There is no doubt that philologists, who do not consider the principles of the new methodical nomenclature, may, at first sight, think the term *arsénic* objectionable; particularly as previous custom and analogy had given another denomination, *arsenical*, which is the natural adjective of the substantive *arsenic*. They may say, that the difference of accentuation alone marks the distinction between the substantive and the new adjective. But every chymist will set the weightier considerations of method and order before such objections. In French, the termination in *ic*, for

and, in order that every particle of water might be finally expelled, to prolong it. When the diminution of weight was ascertained, the residuum was dissolved in acetous, or, still better, in dilute nitric acid, and nitrate of lead was poured in. Arseniate of lead and nitrate of copper were thus formed, by double decomposition; but, when more nitric acid had been used than was strictly necessary to dissolve the arseniate of copper, no precipitate appeared, till the liquor had been evaporated. When the evaporation was pushed too far, part of the nitric acid, contained in the soluble nitrate of copper, flew off; and that nearly insoluble cupreous nitrate, first mentioned by Mr. PROUST,* was produced. To obviate both inconveniences, alcohol was added, immediately before the liquor was quite evaporated, and long after the precipitate had begun to appear; in a few minutes, all the arseniate of lead fell to the bottom, while the nitrate of copper was held in solution. These new products being separated by filtration, the spirituous liquor was distilled; and, from the nitrate of copper, the quantity of

the substantive, and in *ique*, for the adjective, obviates all confusion. One remark I shall beg leave to offer to the consideration of those chymists, who have laboured to adapt to the English language, a literal translation of the French nomenclature. It is the genius of the former language, to throw the accent as far back as possible; so that, in trisyllabical nouns, the first or second syllable is usually accented; while, in the French language, the accent is generally thrown upon the last: thus, we say *sulphúric acid*, but they say, *acide sulphurique*. It was very natural therefore, as in the latter case, to make the accented syllable be that, which should denote the particular state of the substance of which they speak. Thus *sulphurique, sulphureúx*; *nitrique, nitreúx*; *sulphâte, sulphite*; *nitráte, nitrite*. But, without offending the radical orthoepy of our language, we cannot make the same method subservient to that purpose; for, when we wish to mark the distinction in that manner, we are obliged to wrest the word from its proper pronunciation, and to say, *nitríc, nitróus, sulphureoús*, &c.

* Annales de Chimie, Vol. XXXII. p. 26.

that metal contained in the ore was obtained, by boiling the solution with pot-ash or soda.*

* By potash and soda, I mean those alkalis pure, obtained according to the method prescribed by BERTHOLLET. I know of no other. It is not, that I have any predilection for those identical terms; yet, whatever melioration subsequent improvement may introduce in particular cases, if principles are to be adopted, they should, in general, be strictly adhered to. But it must be a violation of them, to apply a word, appropriated by common consent to design a pure, and as yet a simple substance, to such heterogeneous mixtures as lapis causticus, carbonates of potash and soda, &c. It is indeed much to be desired, that the epithets, *caustic*, *pure*, *saturate*, &c. should be regarded as tautology, which they really are. There is no potash purer than potash. When it is not pure, we should say, instead of "I took so much potash," "I took so much of a mixture of potash, and, whatever other substance is mixed with it." Thus, instead of calling lapis causticus, caustic potash, or potash, as is often done, we should say, "I took so much of a mixture of potash, sulphate, muriate, carbonate, and sulphuret of potash; siliceous and aluminous earths; iron and manganese;" for such I find, by analysis, lapis causticus to be. To all this is added, by apothecaries, a little lime. Yet this is the substance sometimes called potash.

M. LOWITZ's manner does not give potash pure enough for delicate analyses of stones. I have never seen any prepared by his method, in which I could not discover iron, silica, alumina, and carbonic acid. To the proofs given by Dr. KENNEDY, (in his paper entituled "A Chymical Analysis of three Species of Whinstone, and two of Lava, in the Edinburgh Transactions for 1799,) of the efficacy of his method, I propose the following objections. That chymist supersaturates by nitric acid, and examines by nitrates of barytes and of silver. This will be a sufficient test for sulphuric and muriatic acids; but carbonic acid may have been present before saturation. He then evaporates; and, if all is redissolvable, concludes there is no silica or alumina; but, after saturation by an acid, ammonia is a more delicate test than evaporation, for small portions of those earths.

By treating Dantzic potash, or, still better, pearlash, with lime, and evaporating in a well-plated copper vessel, a white mass is left. This mass, dissolved as far as it can be in alcohol, and the liquor distilled to dryness in a plated alembic, gives an alkali of a perfect whiteness. In this state, it is dangerous to touch it; its action on animal matter is so sudden, and so violent. It attacks all stones with the greatest ease and rapidity. Dissolved in water, it makes not the least cloud in barytes water, or in a solution of nitrate or muriate of that earth; and may be used, as a very delicate and sensible reagent, to distinguish it from strontian. By saturating with an acid, and then

To the use of alcohol, in order to get rid of the excess of acid, as mentioned above, there is not the same objection that there might be to evaporation, or to an alkali: it can combine with that acid only which is free; and an excess of it can, in no way, affect the metallic salts.

I have given the preference to lead, above every other method of combining arsenic, to determine its quantity in any other body, having found arseniate of lime, which has been hitherto recommended, as well as all other earthy arseniates, to be nearly as soluble in water as sulphate of lime. Lead presented also much facility as to the proportions of its arseniates; and a few experiments, instituted to arrive at them, afforded sufficient accuracy. But, first, it was necessary to ascertain how much acid a given quantity of metallic arsenic could afford; and, finding that it was in vain to aspire at a greater degree of precision, than that which Mr. PROUST had obtained, I have adopted his results. By them it appears, that 133 of white oxide, and 153 of acid, contain each 100 of real arsenic, the rest being oxygen. But, 100 of metallic arsenic, acidified by nitric acid, neutralized by an alkali, and precipitated by nitrate of lead, gave 463 of arseniate of lead; that is, 100 of arseniate of lead, contain 33 and a fraction of arsenic acid; and, on the other hand, my own experiments informed me, that lead, dissolved in nitric acid, and precipitated by arseniate of ammonia,

seeking silica, or alumina, by ammonia, no trace of them can be found, nor indeed of any thing else. I do not say, however, that the potash is perfectly free from every other substance; I believe it contains a little carbone, produced by the decomposition of the alcohol, and is, therefore, a subcarburet of potash; but carbone can be of no consequence, in the generality of experiments, in humid docimasia. The same method, employed with carbonate of soda, is the only one to procure soda in a state of equal purity.

gave a proportion of 63; and 4 were expelled by heat from this salt. The composition of arseniate of lead, therefore, is,

Arsenic acid - - - - -	33
Oxide of lead - - - - -	63
Water - - - - - -	4
	100

This experiment, repeated several times, never gave 1 per cent. difference in the results. Another method, which may be deemed shorter, and perhaps even more accurate, to analyze arseniate of copper, is as follows. After the quantity of water has been estimated, the remainder may be treated by either of the fixed alkalis, which will combine with the acid, and leave the brown, the only real, oxide of copper, in the same state as that in which it existed in the ore; the alkaline liquor may be neutralized, as above, and the proportions determined in the same manner.

No. I. *Third species of the preceding Paper.* One hundred parts, exposed to a low red heat, lost nothing of their weight. Dissolved in dilute nitric acid, decomposed by nitrate of lead, and precipitated by evaporation, and then by alcohol, they left a white powder, which, well washed and dried, weighed 121. But 121 of arseniate of lead, contain 39,7 of arsenic acid. The nitrate of copper, boiled with potash, left a precipitate, which weighed 60. Therefore, there are in this variety,

Oxide of copper - - - - -	60
Arsenic acid - - - - - -	39,7
	99,7

No. II. *Fourth species of the preceding Paper.* One hundred parts, exposed to a low red heat, lost 16. Treated as above,

they yielded a quantity of arseniate of lead, corresponding to 30 of arsenic acid; and I obtained 54 of oxide of copper. Therefore, this variety contains,

Oxide of copper - - - - -	54
Arsenic acid - - - - - -	30
Water - - - - - - - -	16
	100

No. III. *Var.* 2 *of the third species.* One hundred parts, exposed to a low red heat, lost 18 of water. The 82 remaining, boiled with potash, left a residuum of a blackish brown colour, which weighed 51, and which, examined by the different reagents, was found to be oxide of copper, without mixture. The supernatant liquor, and the liquor which washed the 51 precipitated, being neutralized and evaporated together, left a precipitate, by nitrate of lead, which weighed 88, and, by the proportions of arseniate of lead, established above, indicated 29 of arsenic acid. The proportions in this variety are therefore as follows:

Oxide of copper - - - - -	51
Arsenic acid - - - - - -	29
Water - - - - - - - -	18
	98

No. IV. *Var.* 5 *of the third species.* This gave, by one or other of the two methods, already described and applied, as follows:

Oxide of copper - - - - -	50
Arsenic acid - - - - - -	29
Water - - - - - - - -	21
	100

No. V. *Second species of the preceding Paper.* This is the variety which, according to the description I received from M. VAUQUELIN, he had analyzed. In his letter to me, he gave no particulars of the method he had employed, but merely stated his result.* By that, it appears to contain,

Oxide of copper - - - -	59
Arsenic acid - - - - -	41
	100

Before the reception of his account, I had found,

Oxide of copper - - - -	58
Arsenic acid - - - - -	21
Water - - - - - - -	21
	100

This induced me to repeat the analysis with the greatest care and attention; for I thought, that to differ from so great a master must be to differ from truth; but I constantly found 21 of water, and 21 of arsenic acid.

This apparent difference must, therefore, depend on the state of dryness in which he obtained his acid; or perhaps he estimated it with the water; and, if so, I am happy to find I agree with him so near as one per cent. A greater precision, as every person familiar with analysis well knows, is not within the power of chymical exactness.

* J'ai analyzé, ces jours derniers, une mine de cuivre d'un vert clair cristallin, en lames hexaëdres, se divisant en lames menus, et legèrement flexibles, comme le mica; et c'est pour cela, que les naturalistes l'avoient nommé *mica vert.* J'ai trouvé que ce mineral étoit composé d'environ 59 d'oxide de cuivre, et de 41 d'acide arsenique; et c'est de véritable arseniate de cuivre. Paris, August 30, 1798.

No. VI. *First species of the preceding Paper.* One hundred parts, exposed to a low red heat, lost much more than any of the other kinds; the deficit amounted to 35. The usual treatment gave 49 of oxide of copper, and only 14 of arsenic acid. I repeated this analysis, with some portions which had not been exposed to heat, and never found more than 14 of arsenic acid. This arseniate contains,

Oxide of copper - - - - -	49
Arsenic acid - - - - - -	14
Water - - - - - - -	35
	98

No. VII. *Yellow hematitic copper ore.** (See page 171.)

One hundred parts of this ore, boiled with dilute nitric acid, left a yellowish white residuum, which weighed 17. These 17, exposed to a degree of heat sufficient to volatilize the sulphur, left 5, which were silica. The liquor from which this residuum had been separated by filtration, upon being tried for all the different metals, and particularly for arsenic, afforded no traces of any thing but copper and iron. A copious precipitate took place by the affusion of ammonia; the copper was redissolved by adding an excess; and then obtained by volatilizing that alkali, and boiling with potash, after the filter had separated the iron already precipitated. The contents are,

Sulphur - - - - - - - - -	12
Silica - - - - - - - -	5
Copper, which I believe to be in the metallic state	30
Oxide of iron - - - - -	53
	100

* This and the following (No. VIII.) being the matrices upon which the arseniates of copper and of iron are generally found, I thought it right to give an analysis of them also.

In this ore, I believe, for the following reasons, that the metals are in the state I have marked. First, the proportions in the ore announce it; for I always had an excess of weight in the total result, if I did not deduct such a proportion of oxygen as might be contained in 30 parts of copper.

Secondly, there is a considerable disengagement of nitrous gas.

Thirdly, the ore does not attract the loadstone.

And, fourthly, the greater part of the iron, (but none of the copper,) is dissolved in muriatic acid, forming a green muriate of iron, without disengagement of hydrogen gas.

No. VIII. *Grey vitreous copper ore.* (See page 173.) There are many intermediate states between this ore and the yellow hematitic copper ore; but they are not fair objects of chymical analysis, being merely mixtures of both kinds, in different proportions. The mineralogist, indeed, may dwell upon them, as interesting in studying the products of nature, but they are unsatisfactory subjects for the chymist.

Grey vitreous copper ore, when obtained in its greatest purity, is by many degrees the richest cupreous pyrites known in nature; and, in the large way, the metal may be extracted by the easiest processes. 100 parts of this ore, in dilute nitric acid, left 12, which were sulphur. Ammonia, poured in excess into the nitric solution, redissolved, with the exception of 4, the whole of the precipitate which it had formed; the 4 were iron; and, from the ammoniacal liquor, 105 of black oxide of copper, equal to 84 of metallic copper, were obtained by evaporation, and then boiling with potash.

Sulphur - - - - - -	12
Copper - - - - - -	84
Iron - - - - - -	4
	100

Although I have mentioned, in the preceding statements, only a single analysis of each specimen, it is by no means to be imagined, that so small a number would be sufficient to satisfy enquiry. None of the above results have been taken into account, unless confirmed by frequent repetition ; and the probationary experiments have been diversified, as much as lay in my power, by many different tests, and various chymical reagents.

With regard to the colour of some specimens of arseniate of copper, it is easily to be accounted for upon chymical principles. The mistake under which we have long laboured, that the green is the real oxide of copper, has happily been rectified by M. PROUST.* He has proved it to be a particular substance, (to which he has given the very improper name of hydrate of copper,†) endowed with peculiar properties, and composed of the brown oxide, and of water, in a state of combination. From his experiments, and from what I myself have seen, I am inclined to draw the conclusion, that we have never yet obtained by art any real salt of oxide of copper. In examining, for instance, sulphate of copper, we find it to afford blue crystals; and to

* Annales de Chimie, Vol. XXXII. p. 26.

† Copper is not the only metal capable of a similar combination. Cobalt, nickel, and uranium, enjoy the same property. This may, in some measure, explain the change of colour, which the liquid muriates of some of these metals undergo by gentle heat : it may likewise throw some light upon the sympathetic ink of Cobalt.

contain a known quantity of water of crystallization, and of what we formerly called the oxide. But that oxide still retains a quantity of water, of which when it is deprived, it passes to a very dark brown, and changes its chymical nature and properties.

If, upon that brown oxide, a sufficient quantity of dilute sulphuric acid is poured, it yields a blue salt, but in a proportion greater, by about 24 per cent. than if the green substance had been employed. I imagine, therefore, that the first operation of this brown oxide is, to assume the quantity of water necessary to constitute a hydrate; and, that the combination of sulphuric acid takes place, not between the oxide, but between the hydrate and that acid, to form a salt, which, when crystallized, has taken another portion of water in the act of crystallization. It is a well known fact, that there is a state of concentration, when an aqueous solution of muriate of copper, gently heated, will change from a bluish green to a beautiful brown, which, upon cooling, or by the affusion of water, resumes its former tinge. This brown liquor is probably a solution of muriate of copper; while the blue liquor, like every green or blue solution of a cupreous salt, is a combination of the acid and the hydrate, or (as we should say in this case) a muriate of hydrate of copper. It is true, I have not been able to produce, so often as I could wish, this change of colour. I can, however, adduce the following instance, as being much in favour of my opinion.

It is evident that oxide of copper (for so I shall henceforth call the brown substance) has a very strong affinity for water; because the fixed alkalis, (unless when boiled upon it,) and their carbonates, all of which easily decompose the salts of copper, cannot dispossess the hydrate of copper of its water.

This led me to imagine that I could perhaps, by fire, dissipate not only the water of crystallization, but that contained in the hydrate; and leave the acid, if a potent one, still in the salt. But I found that the affinity of the alkali, acting upon the acid, had, in the humid way, determined an order of combination not to be effected by heat; for, even sulphuric acid was expelled, before the water of the hydrate could be completely dissipated. Upon reflecting on the fixity of acids, I could find none so proper for this experiment as the phosphoric. I therefore prepared some artificial phosphate of copper, by precipitating the nitrate of that metal by phosphate of soda. When washed and dried, it was in the form of a fine bluish green powder, among which, many crystals were discernible, almost to the naked eye.

One hundred parts of this, exposed to a gentle red heat, became of a much paler green, but passed intirely to brown when the temperature was sufficiently elevated. I had then a brown phosphate, not of hydrate, but of oxide of copper, and from which no acid had been volatilized. Its loss of weight was wholly from the water which had been expelled, and amounted to 15,5. Its other proportions I found, by further analysis, to be 35 of phosphoric acid, and 49,5 of oxide of copper. It is not, however, to be concluded from this, that there are really 15,5 of water of crystallization, in bluish green phosphate of copper. We must recollect, that it is a phosphate of hydrate of copper; and that 49,5 of oxide demand 12 of water, to exist in that state: 3,5 therefore are the amount of the water of crystallization; and its order of union may, with more propriety, be thus stated:

Oxide of copper	49,5	forming hydrate of copper	61,5
Water - -	12		
Phosphoric acid - - - - -			35
Water of crystallization - - - -			3,5
			100,0

And this is the order which should be adopted, in the statement of all analyses of salts of copper.

I could easily produce, by the same method, a pale green, or a brown arseniate; and, in nature also, the colour of the ore accurately corresponds with the proportion of water, as may be seen by comparing together any of the foregoing analyses.

Having thus convinced myself, by analysis, that copper is found in nature united with arsenic acid in different proportions, I next wished to ascertain, whether art could effect similar combinations. For this purpose, I poured into arseniate of ammonia, a solution of nitrate of copper. The metallic arseniate was immediately precipitated in crystalline grains, of a blue colour, rather more intense than the phosphate already mentioned; and the liquor, which remained blue, was decanted. The colour which this latter retained, I imagined, was due to the presence of a greater quantity of nitrate of copper than was necessary to precipitate, from its alkaline basis, the arsenic acid combined with the copper. After a partial evaporation, I poured in alcohol; and found, to my surprise, that the consequence was another precipitation, which was much increased by allowing the liquor to remain. Crystals, still more rich in colour than the former, and very evidently rhomboidal, even to the naked eye, were gradually formed. Imagining there must be

some essential cause of the greater solubility of the one than of the other, I resolved to examine them apart.

One hundred parts of the first of those precipitates, exposed to a low red heat, lost 22. Boiled with potash, there remained undissolved, a blackish brown powder, which, well washed and dried, weighed 50. The supernatant liquor, saturated with nitric acid, and evaporated, was precipitated by nitrate of lead. Upon filtration, 82 were left, which indicate 27 of arsenic acid. Therefore, this arseniate of copper contained,

Copper - - - - -	50
Arsenic acid - - - -	27
Water - - - - -	22
	99

The second artificial compound, which was mentioned above, was evidently more soluble than the latter; and analogy might lead us to suspect, in arguing from the generality of salts the basis of which is supersaturated, that it contained an excess of acid. It was analyzed in the same manner as the last, and afforded,

Oxide of copper - - -	35
Arsenic acid - - - -	39,5
Water - - - - -	24
	98,5

Thus then have we two artificial arseniates of copper, one of which intimately corresponds with one of those which we have recognized among the productions of nature. The other possibly will be found, but we are not yet in possession of it; for I shall presently mention the reason why No. I. although containing 39 of acid, cannot fairly be esteemed as such. I

have not yet been so fortunate as to form the other combinations; but do not doubt, that art may one day succeed in obtaining them.

REMARKS.

Before I conclude this section, which hitherto has had for its object a particular account of certain kinds of copper ore, as well as of their matrices, it may not be superfluous to offer a few remarks, not foreign to the present subject, upon some methods generally used in the docimastic art. To prove the presence of different substances in fossils, is an object of delicate research; but, to determine proportions with accuracy is the most difficult operation of analytic chymistry, and often eludes investigation. It is rather a pleasing reflection to think, that we are in the infancy of chymical exactness; and that we may see the day of improvement, when the errors which we now commit will require all the aid of self-complacency to be in the least excused. And it may be of more real utility to state with frankness, although we cannot account for them, those anomalous appearances which so frequently occur, than to court the phantom of rigid accuracy, the reality of which we can as yet, but in a few instances, be sure we have attained. For, every observation, however trivial, of this kind, will hasten the arrival of that moment when we shall be enabled to approach a little nearer to truth.

I have, for many reasons, preferred boiling the nitrate of copper with either of the fixed alkalis, to the method generally recommended, which is, to precipitate all the copper from its solvent, by carbonate of potash, or of soda; then, to redissolve in muriatic acid; and to precipitate, in the metallic state, by a plate of polished iron.

First, when an alkaline carbonate is used, the precipitate is a carbonate of hydrate of copper; and this substance is soluble in an excess of the precipitant. I once evaporated some very beautiful blue liquor, obtained in an operation of this kind, and found a crystallized salt, which I became desirous to examine. But, as the solution contained another salt, formed by the acid (which originally held the copper in solution) and the alkali employed, I found it necessary to form some hydrate of copper directly for the purpose.

Some hydrate of copper was therefore prepared, by decomposing the nitrate of that metal by a very dilute solution of potash, and well washing and drying the precipitate: it was that fine powder formerly well known as the oxide of copper. Some of this substance was thrown into a solution of carbonate of potash, through which a current of carbonic acid had been made to pass for a long time, and they were then slightly heated together. One part of the hydrate became of the same colour as the real oxide; the other was dissolved, and the liquor was converted into a greenish blue solution. Thus, one part of the hydrate had yielded its water to the other, in order to favour this quadruple, or rather this double combination, of carbonate of potash and hydrate of copper: the liquor, when reduced, afforded a mass, which, repeatedly redissolved and evaporated, with difficulty assumed any determinate form of crystallization. This salt is a carbonate of potash, holding a little copper. It is of a pale blue, and varies in colour, according to the quantity of water of crystallization, and of metal. It is slightly deliquescent, and soluble in about three parts of water, at 60°, but requires much less water, when the water is boiling. It crystallizes by cooling, much like carbonate of potash. It is soluble

in a large quantity of alcohol. It loses about 43 per cent. of carbonic acid, by solution in a stronger acid; and, prepared in the manner I have mentioned, contains no more than 5 per cent. of oxide of copper; although carbonate of potash, when poured into a solution of any salt of copper, appears to retain a much greater quantity. This, therefore, is a sufficient proof of the inaccuracy of attempting to precipitate copper from its solutions, by an alkaline carbonate; for, carbonate of soda has, like carbonate of potash, the property of dissolving and crystallizing, as a triple salt, with hydrate of copper.

In addition to this source of error, we may add, the very uncertain operation of one metal upon the saline combination of another, whatever may be their affinity for oxygen. Indeed I have so often remarked this very great uncertainty, that I was pleased to find the observation had occurred to Mr. Proust; and I have constantly found that more dependence can be placed upon the method I have adopted, than upon any attempts to precipitate the metals by each other.

With regard to efficacy and promptness, tin or zinc is preferable to iron; but, with any of the three metals, a phenomenon occurs, for which I have not been able to account, in any manner the least satisfactory. As the effects are more striking with zinc than with the other metals, I shall enter into particulars respecting the use of that metal only.

When a bit of zinc (or tin, or iron,) is immersed in a solution of muriate of copper, a precipitation begins, and all the copper is finally separated in a few hours.* But, if a little muriatic acid is added to this solution, and the zinc then immersed, a violent

* If any quantity of neutral salt is present in the solution, the precipitation is much retarded, and is seldom complete.

effervescence takes place; hydrogen gas is disengaged, and, in less than two minutes, the copper is so completely precipitated in the metallic state, that neither ammonia, nor even sulphurated hydrogen, can discover any vestige of its presence.

It would appear from this, that hydrogen is, in this case, the real reductive and precipitant of the copper. Yet, how can we reconcile the evident contradiction that, in one case, zinc with muriatic acid will decompose water, that is, that zinc and muriatic acid will attract oxygen more strongly than hydrogen can, yet that, in the other case, hydrogen, whose affinity for oxygen is weaker than that of zinc and muriatic acid, will be more speedy and powerful than zinc and muriatic acid, in attracting that oxygen from copper. Again, how is it possible that zinc and muriatic acid will, in preference to taking oxygen immediately from the oxide of copper, decompose water, the hydrogen of which will unite with the oxygen of the oxide of copper, again to become water, which it originally was. All this appears to me, I confess, as contradictory as to say, one is less than two, two are less than three, yet three are much less than one. This opinion, that hydrogen is really the reductive, is the more extraordinary, as it is not founded on the single experiment above-mentioned.

If a bit of zinc (or tin, or iron,) is thrown into a solution of oxide of arsenic in water, no change of any kind will be effected, even after a considerable time. But, the instant that muriatic acid is added, effervescence and precipitation commence; a few minutes suffice to obtain all the arsenic in its metallic state. It may be objected here, that muriatic acid, (as muriatic acid,) produces some hitherto unknown modification in the order of established affinities. This objection is not plausible; and I can

adduce so clear an answer to it, as to shew that it is of very little moment. If aqueous arsenic acid is used, instead of the above solution, the same phenomena of effervescence and precipitation ensue, as when muriatic acid had been used in the former case; and that precise quantity of metallic arsenic is thrown down, which can yield sufficient oxygen to the zinc to combine with the remaining acid undecomposed. The precipitate which is formed, is a mixture of metallic arsenic with arseniate of zinc; and these may be separated by muriatic acid, which will dissolve the metallic salt, without acting upon the arsenic.

If, instead of muriatic acid, sulphuric acid is used, the same phenomena take place, in a less degree. But, if the experiment is tried with nitric acid, there is no disengagement of hydrogen gas; and the metals effect a precipitation with much less rapidity and certainty than in the former case.

It is very true, that hydrogen, in its nascent state, may have properties with which we are yet unacquainted; and may determine combinations, which it can in no other state produce. But the decomposition of water, in the first instance, in order that a new recomposition may, at that moment, be effected by the same agents, is in itself sufficiently paradoxical.

The facts last mentioned, are somewhat analogous to, and seem even confirmative of, a theory proposed some time past by Mrs. FULHAME; but I shall withhold my full assent, both to her explanation and to any I could give, until these evident contradictions can in some way be further elucidated.

In the analysis of every ore in which the metal is combined with sulphur, I have found much variation in the quantity of the latter which may be obtained, even in experiments upon the

same specimen. If nitric acid is used not sufficiently strong, part of the ore remains untouched, and will require subsequent treatment, always disadvantageous in delicate operations. If the acid is too strong, a great part of the sulphur is converted into sulphuric acid; so that, in either case, there is room for error. I thought that, to avoid this, (except in cases where any metal which, with sulphuric acid, forms an insoluble salt, was present,) strong nitric acid might be used, and all the sulphur converted into acid. If potash, soda,* or ammonia, are used as precipitants of the different metals, the quantity of sulphur may be easily ascertained. I took a given weight of sulphur, and converted it into sulphuric acid, by means of nitric acid. I then neutralized and evaporated it. Nitrate of barytes, poured in, gave a precipitate which, in one experiment, indicated a proportion of sulphur equal to 14,4, and in another to 14,6, contained in one hundred of sulphate of barytes. A difference so trifling need not be regarded. According to M. LAVOISIER, sulphuric acid contains 71 of sulphur, and 29 ofoxygen; and, according to the synoptic tables of M. FOURCROY, sulphate of barytes contains 33 per cent. of sulphuric acid; therefore, by this calculation, one hundred of sulphate of barytes contain 23,43 of sulphur, instead of 14,4, or 14,6. I do not pretend to account for so great a difference in these results;† but that very difference led me (by exciting me to doubt those which I had obtained, and inducing me to frequent repetition,) to a more positive conviction of the proportions

* See note in page 197.

† I was particularly cautious in ascertaining that, during that experiment, no sulphureous acid had been produced, the formation of which would have easily accounted for any difference.

I have mentioned. M. LAVOISIER obtained his proportions by combustion; and, admitting even that nothing was lost, it must have been rather difficult to obtain the sulphuric acid in a state proper to value the quantity. Indeed I do not know of any direct experiments which prove, in a satisfactory manner, that we have ever obtained that acid perfectly free from water; unless when combined with an earth, or an alkali, in some salt, and that salt calcined in a very strong red heat.

To ascertain the quantity of metallic arsenic in mispickel, arsenical pyrites, &c. the most advantageous method is, to acidify it by nitric acid, and then to combine it with oxide of lead. This arseniate of lead (containing, as was before said, 33,2 per cent. of acid,) may be estimated to contain 22 per cent. of metallic arsenic. If both sulphur and arsenic are present, lead may be equally serviceable, after both have been acidified; for sulphate of lead is not materially soluble in any acid; whereas, on the contrary, arseniate of lead is very much so.

When copper and iron are to be separated, one single affusion of ammonia will not always suffice. That two may be sometimes necessary, is an objection to the method I propose, for the subsequent ebullition with potash. But, when I use that of precipitating the copper by iron, it requires no previous precipitation by any alkali. It is sufficient to add muriatic acid to the original solution of the ore in nitric acid, and evaporate to dryness. The nitric acid is dislodged from the oxide of copper, and the muriatic takes its place. If a single evaporation is not sufficient, a second (for the operation is very short, and causes no loss upon filtres, &c.) may be attempted; and, when the iron, used for the purpose of revivifying the copper, is put in, the liquor may be made to boil; by which means, the process

is rendered much more certain and expeditious. Some iron will necessarily be dissolved; and the quantity must be noted. The liquor, which contained muriate of copper and of iron, now contains only the latter. Boiled with a little nitric acid, it will become red; and then ammonia, or potash, will give a red precipitate, which, well washed and dried, will represent 61 per cent. of metallic iron. All these metals having thus been precipitated, no constituent part of the ore, except the sulphur, which, in the first treatment, had been converted into sulphuric acid, is contained in the liquor; and from it, when neutralized, this latter may be precipitated by nitrate of barytes, which will represent 14,6 per cent. of sulphur. The absolute necessity of constantly using pure alkalis, in this method of analysis, is too evident to be insisted upon.

GENERAL VIEW OF THE FOREGOING ANALYSES.

In taking a retrospective survey of the experiments above related, upon the various natural arseniates of copper which we have examined, we shall find,

First, that natural arseniate of copper exists in three different states of combination; the first containing 14 per cent. the second 21 per cent. and the third about 29 per cent. of acid.

Secondly, that each of these may contain different proportions of water, either as constituting a hydrate, or as water of crystallization.

Thirdly, that, upon losing its water, arseniate of copper will pass from blue to pale green, and finally to brown, as in No. I.

Fourthly, that No. I. is the only real arseniate of copper, all the others being arseniates of hydrate of copper.

Fifthly, That No. I. is not to be admitted as an arseniate of copper containing 39,7 per cent. of acid. For, if we put it on the same footing with the others, in admitting a due proportion of water into its composition, we shall, by calculation, reduce it to that class containing 29 per cent.

Sixthly, That, in beginning with that kind which contains the least quantity of acid, and rising progressively to that which contains the greatest, we shall find the order to be thus:

No. VI. contains - - - -	14 per cent.
No. V. - - - - - -	21 per cent.
Nos. I. III. and IV. - - -	29 per cent.

No. II. seems to be a particular species. It consists of a much greater proportion of oxide, with a less quantity of water, (and this its external colour announces,) combined with nearly the same proportion of arsenic acid. Indeed, if certain characters did not speak so strongly in favour of this division, I should not have hesitated to class it with the last mentioned kinds. But it is found in many states, which seems to indicate, that the water is by no means in the same degree of intimate combination that it is in the others; and this alone may serve to distinguish it, to the eye of the mineralogist.

If, to the above natural arseniates, is added the second artificial arseniate, we shall have another proportion of acid, at the rate of 40 per cent. Here then we have two simple substances, combined in four different proportions, and producing seven distinct combinations.

But, what is not the least to be admired, is the wonderful accordance in the order which two sciences, operating with very different instruments, have allotted to the same substances. By

that, not only the sagacity of Nature becomes very striking; but, from the acknowledged accuracy of one method of investigation, the reliance to be placed upon the other is rendered more conspicuous; and each receives additional strength and confirmation. Chymistry has long been in the habit of aiding the science of mineralogy, of which it laid the foundation; but it was not till lately, that crystallography could form a judgment of its own, much less confirm the truth of the source from which it sprung.

SECTION II.

ARSENIATES OF IRON.

The arseniates of iron remain now to be examined. Included, formerly, among arseniates of copper, they have been separated from them, upon the authority of chymical analysis. For, although to recognize, by external character and form in all their modifications, substances already known, is particularly the province of crystallography; yet he, who would expect that it should declare the nature of those substances which it beholds for the first time, would exact more than it ever has promised, or ever could perform. Among fossils, it may class, and find new species; but chymical analysis is the basis of all arrangement, among metallic ores. In them, to separate, is the task of the one; to assign a place, is the business of the other.

*Cupreous Arseniate of Iron.**

One hundred parts of this arseniate, exposed to a low red

* This species had been mentioned by Mr. PROUST; but in a manner which, as it was a new substance, and demanded particular attention, does not give all the

heat, lost 12, which were pure water. Nitric acid was poured upon the residuum; and, finding that it was dissolved with difficulty, the ebullition was continued during several hours. The liquor was then filtered. 60 parts, which shall presently be examined, remained undissolved. Into this filtered liquor, nitrate of lead was poured, which occasioned a precipitate as usual; but the operation was discontinued, until I should obtain all the arseniate of copper which I imagined to be contained in the ore. For this purpose, I had recourse to the 60 parts mentioned above. They were in the form of a greenish grey powder, very hard and gritty, which had every appearance of silica, contaminated by a small portion of copper interposed between the molecules of that earth. I resolved to treat it in the same manner as all siliceous stones, and proceeded to boil it with potash.

In less than three minutes, it became of a very red brown, from the greenish grey which it originally was; and seemed considerably attenuated in its particles. The liquor was decanted, and examined. It was found to contain arsenic acid; and the precipitate, which had resisted the action of the potash, was proved to be a mixture of iron and copper.

These preliminary experiments were sufficient to indicate a ready method of analysis. 100 parts, boiled with potash, immediately became of a deep reddish brown. The liquor was separated from the residuum by filtration; and, after the usual

satisfaction which that chymist generally affords. No doubt, the scarcity of the ore prevented his making every necessary research; and I may deem myself fortunate in having been so near the spot in which it is found. My friend, Mr. Hatchett, very obligingly gave me a specimen of this ore which he had received from Dr. Pallas, who had brought it with him from Siberia, where it had been found.

neutralization, evaporation, and affusion of nitrate of lead, (all of which operations were detailed in the first part of this Paper,) gave a precipitate, corresponding to 35,5 of arsenic acid. The first residuum weighed 53. Dissolved, as far as they could be, in muriatic acid, there remained 3, which, upon examination, were found to be really silica. Ammonia, poured in excess into the muriatic solution, redissolved 22,5, which were copper; and 27,5 of iron remained behind. The proportions were,

Silica - - - -	3
Arsenic acid - - -	33,5
Oxide of iron - - -	27,5
Oxide of copper - -	22,5
Water - - - -	12
	98,5

None of these experiments were sufficient to determine, whether this ore is in the state of a triple salt, or merely a mixture of two arseniates. As, in a ternary combination, the proportion of acid might vary, it cannot be justly called in to aid us in our enquiry. The solubility of one part of the ore being much greater than that of the other, and in different quantities of each salt, incline more to the opinion, that it is but a mixture.

Simple Arseniate of Iron.

This arseniate, exposed to any degree of heat, gave but an unsatisfactory result, with regard to the quantity of water. The arsenic acid is volatilized from this ore, with peculiar facility, for which I shall attempt hereafter to account.

Some subsequent experiments, however, have induced me to fix the quantity of water at about 10,5.

One hundred parts, boiled with potash, left 58,5. The liquor, treated as usual, by nitrate of lead, gave 31 of arsenic acid. The 58,5 left four, which muriatic acid could not dissolve, and which were silica. Ammonia dissolved 9; and there remained 45,5 of iron. This analysis presents the following result:

Silica - - - -	4
Arsenic acid - - -	31
Oxide of iron - - -	45,5
Oxide of copper - - -	9
Which will leave for Water	10,5
	100,0

This ore appears to be a pure arseniate, mixed accidentally with a little copper; as some of the copper arseniates casually give traces of iron. This is the kind mentioned by Mr. Klaproth, as an arseniate of copper, and the first known under that denomination. Heating it on charcoal, before the blow-pipe, he perceived a smell of arsenic, and, at length, obtained a metallic button, which was found to be copper. That there is copper in this ore, is evident from analysis. But the mere arsenical smell, was not a sufficient ground to assert that it contained arsenic acid; for this metal might, with as much probability, have been in any other state. If indeed, that very accurate and able analyst had, upon trying the ore with the blow-pipe in a platina spoon, perceived no fumes, he might then have concluded, that the arsenic must be in the state of acid, and that charcoal was necessary to operate a partial reduction, to which the arsenic owed its volatility and its smell. But no such experiment is reported.

It is also rather extraordinary, that Mr. Gmelin should have taken this ore out of the class of arsenical ores, and left it as an unknown species of copper; when, in fact, it is an arsenical ore, but not an arsenical ore of copper.

I examined some crystals, which are commonly attached to the specimens of this ore. They were those which, according to M. De Bournon, are in a state of decomposition. By this spontaneous decay, they become of a deep brownish red, not unlike the substance called colcothar; but they still retain their cubic form. They contain a little acid and water, owing perhaps to their having escaped from total decomposition. The same theory that accounts for the difficulty of ascertaining the quantity of water, will account for the red colour they thus assume.

When green sulphate of iron is exposed in a crucible to a red heat, it is well known that sulphureous acid is disengaged in great quantities; and that, if the operation is continued long enough, there remains a red powder. In this case, the green oxide of iron has taken up oxygen, from the acid; and this latter has been partly decomposed, and almost totally volatilized. Now, in the species here spoken of, the iron, as in the green sulphate, is in the state of green oxide; therefore, capable of receiving an additional portion of oxygen. But arsenic acid will, at a high temperature, lose a part of its oxygen, and, retrograding to the state of white oxide, will be volatilized; and still more easily will those changes take place, when oxide of iron, ready to receive, and arsenic acid, ready to yield oxygen, are in contact. A less degree of heat, therefore, will suffice to drive off this acid, from green arseniate of iron, than from arseniate of copper. But we must not from this conclude,

that the affinity of the latter metal for arsenic acid is superior to that of the former; for, the attraction of green oxide of iron for oxygen, and of caloric for white oxide of arsenic, determine a new order of divelling affinities.

But, most of the mineral acids that have been tried, have been found capable of uniting with iron in two states; in the state of green oxide, and in that of red oxide, the residuary powder above-mentioned. I was desirous to know whether I could, in any manner, imitate by art, the last natural products I have examined, as I had already imitated, in some degree, an arseniate of copper. For this purpose, I decomposed green sulphate and red sulphate of iron, by arseniate of ammonia; and, having well washed and dried the precipitates, proceeded to examine them.

The green arseniate was acted upon by heat, in the same manner as the natural one, and exhibited the same appearances. By the usual methods, I found its proportions to be,

Oxide of iron - - -	43
Arsenic acid - - -	38
Water - - - -	19
	100

This is not the same proportion of acid that is contained in the natural arseniate; however, I state them both as I found them. The other artificial arseniate, which is of a pale greenish red, afforded,

Red oxide of iron - - -	36,5
Arsenic acid - - - -	41,5
Water - - - - -	20,0
	98,0

These salts agree with the generality of the known salts of iron; all of which contain a greater quantity of oxide, as the oxide itself contains less oxygen.

By boiling with nitric acid, it was easy to convert the green arseniate of iron into the red; and such is the case with all the salts of green oxide of iron. As, during the course of these experiments, I had occasion to make some remarks upon the divers habitudes of this metal, which, as far as I know, have not all been observed, I shall terminate the whole of these analyses (as I have already done that part of them which treats of arseniate of copper in all its known varieties) by simply stating what has occurred to me.

I happened to boil some muriatic acid upon a greater quantity of iron than the acid could dissolve. I found a perfectly limpid and colourless liquor remain, which, nevertheless, was a solution of muriate of iron. This colourless liquor being decomposed by arseniate of ammonia, the precipitate was of a greenish white, and soluble in a great quantity of water; but, passing to a much deeper green, quickly fell to the bottom. A prussiate, or a gallate, poured into the said solution, occasioned no change, till it had stood a considerable time exposed to the contact of the air. By potash, and by soda, a white precipitate was thrown down, which quickly assumed a green tinge; and that tinge increased so much as to become a very deep grass green, in a few minutes. Ammonia occasioned a white precipitate, which was redissolved by an excess of the precipitant. The ammoniacal solution assumed the same greenish tinge, and speedily deposited an oxide of iron, which was first of a deep green, but instantaneously became black, with some yellowish ochrey particles on the surface. If, however, these precipitations were

effected in a bottle well stopped, and defended from the contact of the atmosphere, no change of colour took place; and that portion which was dissolved by an excess of ammonia remained in the solution. In endeavouring to distil, or to evaporate, the water of this colourless solution, in order to obtain crystals, it became of a light green, the intensity of which augmented, as the distillation was continued. I could not, therefore, hope to procure this salt in a crystalline form. Hence, it is evident, that we have a white muriate of iron, which, as well as the oxide it contains, is very susceptible of assuming an addition of oxygen; for, to that alone I attribute the precipitation caused in a solution of that salt, into which the different reagents above-mentioned had been poured; a precipitation which did not take place, till after it had been exposed in a situation where it could absorb the quantity of oxygen necessary to produce a change in its principles.

This solution of white muriate of iron, when exposed to the air, becomes green, and is then in the state of green muriate, well known. At a certain degree of oxidation, I have observed the precipitate formed, to be soluble in the carbonates of potash and of soda, and still more so in that of ammonia; but, upon absorption of oxygen, to be quickly abandoned by them, and then to fall to the bottom, in a blackish powder mixed with yellow. If, into a solution of green muriate of iron, nitric acid is poured, both liquors being cool, and not too concentrate, the muriate will become of a blackish brown, not unlike malate of iron. Precipitated by the alkalis, it yields a black powder, no longer soluble in them; but which resembles, in every respect, common black oxide of iron.

If this green muriate of iron is further exposed to the air,

the liquor becomes red, but still gives a blackish green precipitate; but, if it is boiled with nitric acid, it then is converted into a red liquor, which yields a red precipitate, by all the alkalis and earths capable of causing a precipitation. From these observations, upon the different combinations of iron with oxygen, and of oxide of iron with muriatic acid, some conclusions may be drawn, interesting to mineralogy as well as to chymistry. The variety of colour in many stones in which iron has been found, is a fact which, although we cannot deny our assent to direct experiments, has never been accounted for in a satisfactory manner. In white, green, yellow, black, red, in a word, in fossils of every colour, iron, with sometimes the help of manganese, and lately of chromium, has been regarded as the colouring matter of every shade; but it seems almost paradoxical, that the same substance should assume and communicate so many tints. In mica, kaolin, amianthus, asbestos, rock crystal, and all white stones, I believe it to exist as the white oxide; and that state is its first degree of combination with oxygen. In them, this metal is not very abundant; for, if it were, I have some reason (as shall be proved immediately) to think, that they would obey the loadstone strongly. In lapis ollaris, serpentines, and many green stones, we have the green oxide; and most of these are magnetic; nay, as Mr. HUMBOLDT has observed, serpentines enjoy the property of polarity. And thence I conclude, that the rarity of this metal, in the first class, prevents them from participating that quality. This, I believe, to be its second stage of combination with oxygen. In the state of black oxide, it is frequently found, and is too well known to need further comment. I believe this to be its third stage of oxidation. But there is a red liquid muriate, which gives

a very dark greenish precipitate, approaching to dark brown. What the state of that precipitate is, I have not yet determined; but I imagine it to be a mixture of black and red. Brown and yellow oxides, I am confident, are mixtures of simple oxides, and neither of them is an oxide *sui generis*. The red oxide is the extreme of oxidation; and affords many beautiful colours in nature and in art.

It is much to be desired, for the advancement of analytic chymistry, that experiments upon the proportions of oxygen with which metals are capable of uniting, under different circumstances, and upon the combination of those oxides with all the known acids, together with many others of their properties, would attract the notice, and engage the labours, of accurate manipulators. Experiments of this kind have been despised, from an idea of their resembling a mere mechanical employment; but, so far is that from the truth, they may justly be considered among the most difficult problems of chymistry; and it is only from the rigid and constantly similar results of such experiments, that we can hope to attain an intimate knowledge of the principles with which nature has originally operated.

SECTION III.

ANALYSIS OF THE RED OCTAEDRAL COPPER ORE, IN WHICH THE METAL EXISTS IN A STATE HITHERTO UNKNOWN IN NATURE.

In the course of the experiments which have been stated in the preceding sections, I have had occasion to examine a great number of copper ores, and particularly of copper ores from Cornwall; but, the only one which has afforded any interesting

results, is the well known species called red copper ore, crystallized in regular and brilliant octaedrons. It has been so long known, and so often mentioned by mineralogists, that it may excite our wonder when we reflect, that its chymical nature has never been ascertained. For it would be an injustice to that very accurate and scrupulous analyst, M. VAUQUELIN, to suppose, that he meant to pronounce decidedly upon that point, by the single experiment which he had made, * and which is mentioned by the Abbé HAUY, in a short extract of his crystallographical arrangement of mineral substances, published in the *Journal des Mines.*

ROME' DE LISLE, the Baron de BORN, LAMETHERIE, the Abbé HAUY, and indeed every other mineralogist, concur in calling this substance red calx of copper; but some of them assert, that it contains a portion of carbonic acid. Among the many analyses which have been made of this ore, by FONTANA, MONNET, DE BORN, RENOVANTZ, and others, I could not find one, that in the proportions, or even in the ingredients, resembled what I had found to be its contents. The highest amount of copper, (that given by FONTANA,) does not exceed 66 per cent. and is far short of the real quantity. The remainder, as he states, consists of water, and of pure and fixed airs. The difference in the results I had obtained, together with some new facts, which I had occasion to observe during my experiments, induces me to treat the subject at some length; referring for its external characters, to those mineralogists above mentioned, who have amply described the ore, and confining myself entirely to its chymical analysis, and some analogous experiments.

* He merely poured muriatic acid upon the ore; and, as it was entirely dissolved, without effervescence, concluded it to be an oxide, and not a carbonate, of copper.

One hundred parts of very pure and regularly crystallized red copper ore were reduced to a fine powder, and dissolved, without the assistance of heat, in nitric acid.

During the operation, a very violent effervescence, accompanied by a disengagement of nitrous gas, unusually copious and rapid, took place. When these phenomena had subsided, the solution was blue, like every other nitrate of copper; and the ore had entirely disappeared. The liquor, perfectly limpid, was evaporated to dryness: muriatic acid was poured in; and the nitric acid was expelled, by a second evaporation. Into the muriate of copper, which remained behind, a plate of polished iron was immersed, which, after the usual phenomena, gave a precipitate, that was found, upon examination, to be copper, and amounted to 88,5. In order to complete the hundred parts, it would be necessary to add 11,5. But fire expelled from the ore neither water nor any other volatile substance; nor did the weight of a given quantity appear either to diminish or to increase, by long exposure to a moderately elevated temperature. The only oxide of copper with which I was acquainted, as existing in nature, contains 20 per cent. of oxygen. I had therefore, 8,5 of copper exceeding the quantity I should have obtained, had the ore been wholly composed of black oxide of copper. And, on the other hand, as I had convinced myself, that no loss of weight had been occasioned by any part of the metal remaining unprecipitated by the iron from its solution, I could not conclude the ore to be in the state of native copper. I was led, therefore, to imagine, that it might be a mixture of those two substances; and that muriatic acid, by dissolving the one, and leaving the other untouched, would be the most effectual means of producing

the separation I desired, and of determining the proportion of each.

Upon 100 parts of the ore, a sufficient quantity of strong muriatic acid was poured. A total solution was effected, accompanied with disengagement of caloric. The liquor was, at first, of a very deep brown, approaching somewhat to the tinge which water will receive, when strongly impregnated with the colouring matter of dried vegetable substances; but, upon being exposed to the air, and boiled some time, it became like every other muriate of copper; and a plate of polished iron precipitated 88 of metallic copper. From this last experiment it was evident, that no metallic copper was contained in the ore. But still the deficit, to be supplied by oxygen, amounted to no more than 12; while the copious disengagement of nitrous gas, in the first experiment, indicated that the metal was not at its maximum of oxidation; and the rapidity with which it seized upon an addition of oxygen, sufficiently shewed, how strong was the affinity of that principle for copper, in that particular state in which it exists in the ore.

I imagined it would be expedient to attempt some precipitations by other reagents, and make some further experiments. For this purpose, I dissolved some more of the ore in strong muriatic acid; and, when I thought that the acid had taken up as much as it could contain, and that the colour had arrived at its deepest tinge, I gently drew off the clear liquor, using all the precaution which the nature of the experiment allowed, to preserve it from the contact of the atmosphere, and proceeded to examine it. Knowing this solution of muriate of copper to be very concentrate, I attempted to dilute it; but, what was my

surprise, when, upon the first affusion of water, I saw the liquor become turbid and milky, and a very abundant heavy precipitate, of a white colour, fall to the bottom.

Struck with the novelty of this appearance, I proceeded to collect as much of the substance as I could, in order to give it a thorough examination. For this purpose, I decanted the supernatant liquor, and continued to wash the precipitate. Upon every subsequent addition of water, I perceived that the precipitate lost a little of its whiteness, and drew towards an orange colour, not unlike the precipitates of platina. I soon found, therefore, that by this method I had no chance of obtaining, in a permanent and constant state, this muriate of copper, fit to be subjected to experiments proper to determine its internal nature and proportions. I then attempted to make use of alcohol, as precipitant, instead of water; but I found the salt to be soluble in it, when the excess of acid necessary for its solution in water was present. Nor was I more successful, when, after having precipitated by water, I washed with alcohol; for the colour of the salt passed gradually from very white to a shade of orange; less rapidly, it is true, in this case, but still so as to convince me, that I could not even thus procure, in a state constantly similar, the salt I wished to examine. The only conclusion which all these experiments entitled me to draw, was that, in the first instance, water precipitated the muriate of this particular oxide of copper from its solution, but in a manner very different from that in which muriate of antimony, of bismuth, and some other metallic salts are acted upon. When into either of these muriates water is poured, a precipitate ensues, but it retains a very small portion of acid, if any; whereas, in the case before us, it is a salt, and not an oxide of

copper, that is thrown down. In order to effect, in this salt, a decomposition similar to that which takes place in muriate of bismuth, or of antimony, it is necessary to draw off the first liquor, and then proceed to wash copiously. The precipitate will by degrees assume an orange colour, which, as we shall presently see, is the real appropriate colour of this oxide of copper, prepared in the humid way.

It is evident also, from this precipitation, that this oxide of copper combines with muriatic acid by a very slender affinity.

As it did not appear to me, that I should obtain any thing very satisfactory from this combination with muriatic acid, I resolved to try some other acids. Sulphuric, phosphoric, oxalic, citric, acetic, tartareous, and acetous acids, were each poured upon known quantities of the ore, and kept in bottles completely filled and well stopped, in order to prevent any absorption of atmospheric oxygen. The liquors generally became blue; and, upon trial, were found to contain the common and well known salts of copper, composed of the respective acid, and the oxide of copper containing 20 per cent. of oxygen; while a large portion of the ore appeared to remain in its original state. But, as I was certain, that there could be no decomposition in most of these acids, under the above circumstances, and moreover, that no oxygen could be taken in from the atmosphere, it became a matter of no smallinte rest, to examine from what source the metal dissolved had acquired the necessary quantity of oxygen to favour its solution, and afford the usual salt of copper, in which it is oxidated in the proportion of 20 per cent.

I repeated, with all the above acids, the experiments tending to satisfy that enquiry; but, as the results from all were nearly

similar, I shall mention that only which proved to be the most ample, and the most conclusive.

One hundred parts of the pulverised ore were introduced into a small phial, and dilute phosphoric acid was poured in, so as to fill it. A ground stopper closed it completely; and, in that state it was suffered to remain three days, during which time the bottle was frequently shaken. The acid became at first of a light blue, and increased in colour by remaining upon the ore. At the expiration of the above term, the liquor was decanted; the residuum was well washed and dried, and weighed 42. The blue liquor contained merely common phosphate of copper, held in solution by an excess of acid. Upon the 42 parts of residuum, strong muriatic acid was poured, which did not appear to produce the smallest change or effect. It was evident, therefore, that some previous alteration had been produced; for, if it had remained in its original state, muriatic acid would have acted upon it, as in the case already mentioned. To operate more effectually, nitric acid was added, and the whole gently heated. A complete solution followed, during which, much nitrous gas was disengaged. The remainder of the nitric acid was expelled by evaporation; and a plate of polished iron, immersed in this muriate of copper, afforded a precipitate of metallic copper, weighing within one part as much as the weight of the first residuum. It was evident, therefore, that a partial reduction of the ore had taken place; and, what is still more strange, had taken place by means of the presence of an acid.

In many observations which have presented themselves, in the course of various analytic experiments, something similar had before occurred to me. I have known metallic oxides yield

a part of their oxygen, one to the other, in favour of some particular solvent. When the metallic oxide *A*, for instance, containing 25 per cent. of oxygen, is in contact with the metallic oxide *B*, containing 10 per cent. they will each remain quiescent in their respective states. But, if the solvent *C* comes to be added, and if the substance *B*, at 10 per cent. of oxygen, has no affinity for *C*, but at 15 or 20 per cent. has a very powerful affinity for it, then may the oxide *A* lend a part of its oxygen, in order to favour the combination of *B*, at 15 or 20 per cent. with the solvent *C*. Indeed, as soon as I saw the phosphoric acid assume gradually a blue tinge, and the undissolved powder begin to wear a more brilliant appearance, I imagined I should not fail to recognize the same fact in this case. When phosphoric acid has remained long enough upon the pulverized ore to dissolve all it can, the oxygen is concentrated, as it were, to the amount of 20 per cent. in the part which is dissolved; and all that which could not be dissolved has (through the two-fold affinity of copper for oxygen, to the amount of 20 per cent. and of phosphoric acid for that oxide of copper, at that degree of oxidation,) yielded up its entire share of oxygen, to favour the combinations which take place in a new order, the only one which can exist among the substances now present. It is, therefore, to the disposing affinity,* caused by the presence of the phosphoric acid, which seeks to combine with black oxide of copper, that the reduction of 42 per cent. of this ore is entirely

* As the term *predisposing affinity* has been objected to, I have used the term *disposing*, which I trust will not be thought improper. When in two bodies which, while together, remain in their original state, the equilibrium of their principles comes to be broken by the presence of a third, we cannot but allow, that it is this third which has disposed them to the rupture of that equilibrium; and, most certainly, be the fact explained as it may, whatever disposes may be called disposing.

to be attributed. All the acids above mentioned are capable of producing the same change, but in a manner, perhaps, not quite so distinct or satisfactory.

From the foregoing experiments it appears, that copper exists in this ore in a state hitherto unknown in nature; and that it contains much less oxygen than has ever been suspected in any oxide of copper; for, from the quantity which was precipitated in the metallic state by iron, it appears to be combined in the proportion of about 11,5 per cent. To confirm this idea, and ascertain as nearly as I could the precise quantity, I dissolved 100 parts in nitric acid; then boiled with potash, and filtered. One hundred and eleven remained upon the filter, which, as they had combined with a new portion of oxygen from the nitric acid, were in the state of black oxide, and correspond exactly to 88,75; so that I believe I shall be within one per cent. of the truth, in asserting the proportions to be,

Copper - - - -	88,5
Oxygen - - -	11,5
	100,0

When, into a solution of muriate of suboxide of copper, liquid potash or soda is poured, a bright yellow precipitate, not unlike the precipitate of platina, takes place. This precipitate differs only in colour from the original ore; for it is soluble in muriatic acid, and affords the same solution and precipitation by water, and the same appearance with alcohol. It is likewise soluble in nitric acid, but with disengagement of nitrous gas, and gives the same appearances with the other acids above enumerated. The difference of colour seems to arise merely from the tenuity of its molecules, compared to the mechanical pulverisation of

the natural oxide. When alone and dry, it is much more permanent in its nature, than when combined with muriatic acid; but any part of it that happens to be in contact with a filter, becomes green, and then blackish, leaving a mark of the same shade upon the paper. Were it not for this property of changing, it might be of use in the art of painting; for the colour is extremely beautiful, and would be highly valuable, if durable.

The precipitate caused in the muriate of copper, by the carbonates of potash and soda, is of a brighter yellow, and is a real carbonate of suboxide of copper. But, if ammonia is poured, at first in a small quantity, into the above solution, the precipitate is blue; and, upon adding an excess of the precipitant, the whole is redissolved, and the liquor is like any other ammoniacal solution of copper.

In order, however, to determine in what state the copper was dissolved by that alkali, I poured some ammonia upon 100 parts of this suboxide, in a well-closed phial. The liquor became blue; and I expected to find that part of the ore had been reduced, as with phosphoric acid; but the residuum was entirely soluble in muriatic acid, with the usual phenomena.

A spirituous tincture of galls, poured into muriate of suboxide of copper, afforded no precipitate, owing, I suppose, to the excess of acid; but, sulphurated hydrogen gas threw down a black, and prussiate of ammonia a lightish brown, precipitate.

I endeavoured to obtain muriate of suboxide of copper, by evaporation, and by distillation in a retort; but, as I could perceive the liquor constantly assume a bluish tinge, I could not reckon upon the purity of the salt, sufficiently to submit it to analysis.

Such were the principal experiments, which the fleeting and precarious existence of the salt allowed me to make upon it. But, from some properties which I had remarked, I could perceive that this ore was a natural oxide of copper, nearly in the same state as that artificial oxide which M. Proust had found in the white muriate of copper, obtained by pouring a recent solution of muriate of tin into a solution of muriate of copper.

If, however, by the very nature of the substance, (which, as I saw it ever changing, I thought it would be loss of time to examine farther,) I have been turned aside from more certain results, I have been more successful in imitating by art the state of this natural product.

By exposing oxide, hydrate, or carbonate of copper, without addition, to a violent heat, in an open crucible, I frequently obtained the suboxide, which then presented all the properties already recognized in the above species of copper ore. In one instance, I so far succeeded, that, upon the very first inspection, the well experienced eye of the Count de Bournon, recognized a lump of it to be a mass of semi-fused, artificial, red copper ore.

But I have found a method of producing at pleasure, in the humid way, all the new salts, and the oxide above described. As I had found about 11,5 per cent. of oxygen to be the quantity contained in the ore, I took that quantity of black oxide of copper which corresponded to 11,5 of oxygen; (57,5 of black oxide was the proportion thus indicated;) on the other hand, I took 50 parts of metallic copper, which had been precipitated by iron from muriate of copper, and which was in a state of tenuity not inferior to the finest powder. These were well mixed, by trituration in a mortar, and put, with muriatic acid,

small mixture of any disoxidating substance would, in a short time, reduce immense quantities.

From the foregoing experiments we may perceive, into how many errors we may be drawn, if, in arguing from the results which we obtain, we pronounce too hastily upon the state in which a substance exists, in the subject of any analysis. After what has been shewn, with regard to the action of muriatic acid upon a mixture of metallic copper and black oxide of copper, both reduced to powder, and of the action of phosphoric acid upon the ore itself, it may be still a doubt, whether this ore is really a suboxide, or a mixture of metallic copper and oxide of copper, at 20 per cent. of oxygen. But, as similar proportions of both, after having been made red hot, presented all the properties and appearances of the ore, much more strongly than when simply mixed, it is fair to conclude, that it is a real suboxide. Had not muriatic acid been used, the natural conclusion would have been, that the ore was a mixture, or at most a combination, of these two substances; for, such did it appear to be, by the testimony of the other acids. The truth is, we are but little acquainted with the exact state in which substances exist, in many natural combinations. However, in the mineral kingdom, such fallacious conclusions are less frequently to be dreaded, than in the vegetable and animal kingdoms. But, in every research, it is important to leave as little room for them as possible; and he who would indicate a sure and constant method of ascertaining whether, in many cases, what we deem a component part is not in fact a product of the operation, would render to science a service, the real value of which is, perhaps, not now entirely foreseen.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

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ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1800.

1800.	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Jan. 1	18	8	0	22	41	30,40	64		ENE	1	Fine.
	25	2	0	25	44	30,27	60		ESE	1	Cloudy.
2	24	8	0	30	41	30,00	78		E	1	Cloudy.
	38	2	0	35	44	29,90	80		E	1	Cloudy.
3	38	8	0	43	43	29,50	87		SSW	1	Cloudy.
	47	2	0	47	47	29,53	90		SW	1	Cloudy.
4	42	8	0	42	47	29,56	93	0,305	SSW	1	Rain.
	47	2	0	47	49	29,54	95		SE	1	Cloudy.
5	40	8	0	40	47	29,47	91		E	1	Cloudy.
	45	2	0	45	51	29,46	87		E	1	Cloudy.
6	36	8	0	36	48	29,48	87		E	1	Foggy.
	41	2	0	41	51	29,55	86		NE	1	Cloudy.
7	39	8	0	39	49	29,68	90		S	1	Foggy.
	41	2	0	41	52	29,69	88		SSE	1	Cloudy.
8	40	8	0	40	51	29,50	80		E	2	Cloudy.
	41	2	0	41	52	29,42	78		E	1	Cloudy.
9	38	8	0	38	51	29,38	88	0,035	E	1	Rain.
	43	2	0	42	53	29,38	89		E	1	Rain.
10	40	8	0	40	52	29,60	88	0,148	E	1	Cloudy.
	40	2	0	40	53	29,67	81		NW	1	Cloudy.
11	38	8	0	38	52	29,65	81		E	1	Cloudy.
	42	2	0	42	53	29,54	83		E	1	Cloudy.
12	37	8	0	37	51	29,26	83	0,035	E	1	Cloudy.
	43	2	0	43	53	29,26	83		E	1	Cloudy.
13	42	8	0	42	53	29,33	87	0,046	S	1	Rain.
	45	2	0	45	55	29,36	83		SE	1	Cloudy.
14	38	8	0	38	52	29,30	82		E	2	Cloudy.
	44	2	0	42	55	29,18	79		E	2	Cloudy.
15	41	8	0	42	53	29,06	86	0,087	NE	1	Rain.
	43	2	0	43	54	29,01	82		W	1	Rain.
16	35	8	0	36	52	29,15	84	0,113	NE	1	Cloudy.
	39	2	0	39	54	29,19	77		N	1	Cloudy.

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for January, 1800.

1800	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Jan. 17	35	8	0	35	53	29,50	80		W	1	Cloudy.
	39	2	0	39	55	29,54	76		SW	1	Fair.
18	36	8	0	39	52	29,05	85	0,077	E	1	Rain.
	45	2	0	44	55	29,05	82		NW	1	Cloudy.
19	37	8	0	45	53	29,08	88	0,385	S	1	Rain.
	45	1	0	41	54	29,07	87		NE	1	Rain.
20	34	8	0	34	52	29,03	85	0,300	NE	1	Snow.
	35	2	0	35	53	29,08	82		N	1	Snow.
21	28	8	0	32	50	29,44	77	0,155	NW	1	Cloudy.
	34	2	0	34	53	29,67	66		W	1	Fair.
22	28	8	0	27	50	29,87	74		E	1	Fine.
	37	2	0	37	52	29,91	74		ESE	1	Fine.
23	30	8	0	30	49	29,83	72		E	2	Fair.
	39	2	0	36	51	29,56	67		SE	2	Cloudy.
24	33	8	0	34	49	29,30	85	0,425	SW	1	Fair.
	40	2	0	40	52	29,39	78		W	1	Fine.
25	33	8	0	34	49	29,77	85		SW	1	Fair.
	44	2	0	43	52	29,81	80		SSW	2	Cloudy.
26	42	8	0	49	50	29,50	90	0,015	S	2	Cloudy.
	51	2	0	51	53	29,50	87		S	2	Cloudy.
27	44	8	0	44	51	29,50	83	0,265	WSW	1	Cloudy.
	45	2	0	44	54	29,72	62		W	1	Fair.
28	35	8	0	35	51	29,93	80		SW	1	Fair.
	46	2	0	46	53	29,78	73		SW	1	Cloudy.
29	40	8	0	40	51	29,42	76		SW	1	Cloudy.
	44	2	0	44	54	29,31	68		SW	1	Fair.
30	32	8	0	32	50	29,36	73		S	1	Fine.
	42	2	0	42	54	29,40	68		SW	1	Fine.
31	34	8	0	35	51	29,45	77	0,067	E	1	Cloudy.
	41	2	0	41	53	29,43	74		SSE	1	Fair.

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for February, 1800.

1800	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
Feb. 1	32	7	0	32	50	29,55	81		S	1	Foggy.
	46	2	0	46	53	29,46	80		S	2	Cloudy.
2	33	7	0	33	50	29,93	76	0,057	SW	1	Fair.
	44	2	0	44	52	29,98	73		SW	1	Cloudy.
3	42	7	0	47	51	29,75	86	0,018	SW	2	Cloudy.
	47	2	0	47	53	29,90	64		SW	1	Cloudy.
4	36	7	0	36	50	30,15	74		SW	1	Fine.
	45	2	0	45	54	30,24	62		W	2	Fine.
5	32	7	0	32	51	30,27	77		E	1	Cloudy.
	38	2	0	38	54	30,25	74		E	1	Cloudy.
6	32	7	0	32	50	30,24	80		NE	1	Foggy.
	35	2	0	35	52	30,26	79		NE	1	Cloudy.
7	28	7	0	28	50	30,22	75		ENE	1	Cloudy.
	36	2	0	36	52	30,22	67		ENE	1	Cloudy.
8	31	7	0	31	48	30,22	60		E	1	Cloudy.
	34	2	0	34	52	30,23	59		E	1	Cloudy.
9	28	7	0	31	47	30,18	67		E	1	Cloudy.
	36	2	0	36	50	30,18	64		E	1	Cloudy.
10	32	7	0	32	47	30,06	69		E	2	Cloudy.
	36	2	0	36	51	30,00	66		E	2	Fine.
11	30	7	0	30	46	29,92	72		E	2	Fine.
	36	2	0	36	50	29,92	64		E	2	Fine.
12	25	7	0	26	47	29,88	71		E	2	Fine.
	32	2	0	32	48	29,86	66		E	2	Fair.
13	30	7	0	30	45	29,72	67		NE	2	Cloudy.
	32	2	0	32	47	29,72	67		NE	2	Cloudy.
14	31	7	0	31	46	29,93	66		NE	1	Cloudy.
	32	2	0	32	47	29,96	66		NE	1	Cloudy.
15	30	7	0	30	46	30,02	71		NE	1	Cloudy.
	35	2	0	35	49	30,06	70		NE	1	Cloudy.
16	32	7	0	35	46	29,94	76	0,108	E	1	Rain.
	41	2	0	41	49	29,93	83		E	1	Cloudy.

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1800	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Feb. 17	38	7	0	38	47	29,86	90		E	1	Foggy.
	40	2	0	40	50	29,77	87		E	2	Cloudy.
18	34	7	0	34	47	29,80	81		E	1	Cloudy.
	41	2	0	41	50	29,84	71		E	1	Cloudy.
19	32	7	0	32	47	29,88	86			0	Foggy.
	40	[illegible]	0	40	50	29,82	81		E	1	Foggy.
20	32	7	0	32	48	29,67	86		E	1	Cloudy.
	44	2	0	44	50	29,60	74		E	1	Fair.
21	40	7	0	40	50	29,60	80		E	1	Cloudy.
	51	2	0	51	53	29,62	70		E	1	Fair.
22	39	7	0	40	51	29,53	75		E	1	Cloudy.
	53	2	0	53	54	29,46	74		ESE	1	Fine.
23	39	7	0	39	52	29,31	77		E	1	Cloudy.
	49	2	0	49	54	29,30	68		E	1	Hazy.
24	33	7	0	37	51	29,30	76		NE	2	Cloudy.
	38	2	0	38	52	29,41	74		NE	2	Cloudy.
25	35	7	0	36	50	29,57	78		NE	1	Cloudy.
	40	2	0	40	52	29,61	76		NE	1	Rain.
26	33	7	0	33	50	29,70	77	0,077	NE	2	Cloudy.
	33	[illegible]	0	33	51	29,78	68		NE	2	Cloudy.
27	29	7	0	30	49	29,89	64		NE	[illegible]	Cloudy.
	32	2	0	32	49	29,90	60		NE	2	Cloudy.
28	28	7	0	29	47	29,93	60		NE	2	Cloudy.
	34	2	0	34	48	29,93	57		NE	[illegible]	Cloudy.

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1800	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Mar. 1	27	7	0	28	47	29,98	65		NE	1	Cloudy.
	38	2	0	38	49	30,00	60		NW	1	Cloudy.
2	31	7	0	31	46	29,96	72		SW	1	Cloudy.
	41	2	0	41	50	29,90	57		W	1	Fine.
3	32	7	0	32	46	29,78	76		NE	1	Fair.
	40	2	0	40	50	29,78	60		NE	1	Fair.
4	34	7	0	33	47	29,78	68		NE	1	Cloudy.
	39	2	0	39	50	29,82	59		NE	1	Cloudy.
5	28	7	0	28	47	29,88	69		NE	1	Fair.
	35	2	0	35	50	29,88	64		ENE	1	Fine.
6	25	7	0	27	46	29,88	70		NE	2	Cloudy.
	32	2	0	32	48	29,87	61		ENE	2	Cloudy.
7	23	7	0	23	45	29,84	61		E	2	Fine.
	32	2	0	32	47	29,74	60		E	2	Fine.
8	25	7	0	27	45	29,39	66		ENE	2	Fine.
	35	2	0	35	48	29,61	57		ENE	2	Fine.
9	26	7	0	28	46	30,00	65		NE	1	Cloudy.
	36	2	0	36	47	30,05	60		E	1	Cloudy.
10	30	7	0	30	45	29,93	63		ENE	2	Cloudy.
	38	2	0	38	47	29,88	61		ENE	2	Cloudy.
11	35	7	0	41	46	29,70	84	0,067	ESE	1	Rain.
	53	2	0	53	49	29,68	88		SSE	1	Cloudy.
12	39	7	0	39	49	29,52	86		E	1	Cloudy.
	42	2	0	42	50	29,70	84		W	1	Rain.
13	33	7	0	33	49	30,08	84		NE	1	Foggy.
	40	2	0	40	51	30,12	81		E	1	Cloudy.
14	31	7	0	34	49	29,98	76	0,120	E	2	Cloudy.
	42	2	0	42	51	29,89	62		E	2	Cloudy.
15	37	7	0	37	49	29,73	64		E	2	Cloudy.
	46	2	0	46	52	29,73	60		NE	2	Cloudy.
16	34	7	0	34	50	29,85	78		NE	1	Cloudy.
	47	2	0	47	53	29,90	70		NE	1	Fine.

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1800	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Mar. 17	35	7	0	36	50	30,00	75		NE	1	Cloudy.
	39	2	0	39	53	30,03	72		NE	1	Cloudy.
18	36	7	0	36	50	30,01	70		NE	1	Cloudy.
	39	2	0	39	52	30,01	69		NNE	1	Cloudy.
19	31	7	0	32	50	30,00	74		NE	1	Cloudy.
	41	2	0	41	53	30,01	65		NE	1	Cloudy.
20	31	7	0	32	49	30,13	70		NE	1	Cloudy.
	45	2	0	45	50	30,15	66		NE	1	Cloudy.
21	38	7	0	38	50	30,13	69		NNE	1	Cloudy.
	49	2	0	49	53	30,13	59		NNE	1	Cloudy.
22	38	7	0	38	49	30,18	70	0,025	NE	1	Cloudy.
	46	2	0	46	52	30,22	66		NE	1	Cloudy.
23	37	7	0	38	50	30,12	69		SW	1	Rain.
	51	2	0	51	51	30,05	68		SW	1	Cloudy.
24	43	7	0	44	50	29,99	80	0,028	SW	1	Cloudy.
	53	2	0	53	53	29,99	78		S	1	Cloudy.
25	36	7	0	38	51	29,99	69		E	1	Cloudy.
	52	2	0	52	56	29,95	54		SE	1	Fine.
26	37	7	0	39	54	29,86	71		E	1	Fine.
	54	2	0	54	57	29,83	58		E	1	Fine.
27	40	7	0	44	55	29,76	69		E	1	Fair.
	55	2	0	54	56	29,71	58		SSE	2	Cloudy.
28	44	7	0	45	55	29,41	70		E	2	Rain.
	49	2	0	49	57	29,28	72		ESE	2	Rain.
29	48	7	0	48	56	29,37	81	0,044	E	1	Rain.
	52	2	0	52	57	29,51	72		SE	2	Cloudy.
30	46	7	0	47	56	29,75	68	0,021	ENE	1	Cloudy.
	57	2	0	57	58	29,81	62		NW	1	Fair.
31	46	7	0	47	56	29,59	76		E	1	Cloudy.
	50	2	0	50	57	29,34	76		SSE	1	Rain.

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1800	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
April 1	44	7	0	44	56	29,27	76	0,580	W	1	Rain.
	53	2	0	53	58	29,44	60		W	2	Cloudy.
2	38	7	0	42	56	29,54	73	0,127	S	2	Cloudy.
	49	2	0	47	57	29,36	76		S	2	Rain.
3	45	7	0	45	56	29,58	74	0,060	NW	1	Fair.
	49	2	0	49	57	29,78	72		N	1	Cloudy.
4	45	7	0	48	56	29,81	75		S	1	Cloudy.
	54	2	0	54	58	29,75	75		S	1	Cloudy.
5	42	7	0	44	56	29,82	73		S	1	Fair.
	56	2	0	56	57	29,78	62		SSW	1	Cloudy.
6	44	7	0	45	56	29,86	72	0,022	S	2	Fine.
	59	2	0	59	58	29,97	53		SSW	2	Fine.
7	49	7	0	50	57	29,98	72		SSW	1	Fair.
	62	2	0	62	60	29,97	56		S	1	Fair.
8	48	7	0	49	58	29,95	68		SSE	1	Cloudy.
	59	2	0	56	58	29,89	64		S	1	Rain.
9	46	7	0	47	58	29,67	75	0,270	E	1	Cloudy.
	56	2	0	56	59	29,58	74		S	2	Cloudy.
10	46	7	0	47	57	29,47	73	0,063	S	2	Rain.
	57	2	0	57	58	29,38	70		SSW	2	Cloudy.
11	41	7	0	43	57	29,66	67	0,082	W	1	Cloudy.
	56	2	0	56	60	29,83	56		WNW	1	Fair.
12	49	7	0	52	58	29,68	80	0,073	SW	1	Rain.
	60	2	0	60	60	29,61	61		SSW	2	Cloudy.
13	47	7	0	48	58	29,73	70	0,060	W	2	Fair.
	55	2	0	53	59	29,76	68		SSW	2	Rain.
14	51	7	0	52	58	29,76	80	0,062	SW	2	Rain.
	57	2	0	57	59	29,86	72		SSW	2	Cloudy.
15	48	7	0	48	58	29,90	78	0,120	SSW	1	Rain.
	60	2	0	60	60	29,90	60		SW	2	Fair.
16	46	7	0	50	58	29,67	75	0,128	S	2	Cloudy.
	57	2	0	55	59	29,53	72		S	2	Cloudy.

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Apr. 17	51	7	0	51	58	29,42	75		S	2	Cloudy.
	55	2	0	55	59	29,49	60		S	2	Cloudy.
18	44	7	0	46	58	29,78	73		W	1	Fair.
	61	2	0	61	60	29,79	50		W	1	Fair.
19	49	7	0	50	58	29,83	73	0,043	W	1	Fair.
	60	2	0	60	60	29,81	56		SSW	2	Fair.
20	49	7	0	50	58	29,70	77	0,042	SSE	2	Rain.
	59	2	0	58	60	29,64	62		S	2	Cloudy.
21	52	7	0	53	58	29,45	73	0,125	S	2	Cloudy. [Much wind last night.
	58	2	0	56	59	29,50	70		S	2	Rain.
22	45	7	0	46	58	29,35	71	0,090	S	2	Cloudy.
	57	2	0	57	60	29,34	61		E	1	Cloudy.
23	43	7	0	45	57	29,33	72	0,028	E	1	Cloudy.
	51	2	0	50	58	29,41	70		S	1	Cloudy.
24	45	7	0	46	57	29,68	70	0,404	NE	1	Cloudy.
	52	2	0	52	58	29,67	66		NE	1	Rain.
25	44	7	0	49	58	29,34	88	0,204	NE	1	Rain.
	51	2	0	49	58	29,42	70		ESE	1	Cloudy.
26	43	7	0	46	57	29,75	78	0,083	E	1	Cloudy.
	60	2	0	59	59	29,79	62		E	1	Fair.
27	45	7	0	45	58	29,88	79		NE	2	Cloudy.
	50	2	0	50	58	29,84	76		NE	1	Cloudy.
28	46	7	0	47	57	29,75	78	0,105	W	1	Fair.
	61	2	0	61	60	29,78	61		SSW	1	Cloudy.
29	45	7	0	46	57	29,67	73	0,088	E	1	Fair.
	58	2	0	58	59	29,63	63		SW	1	Fair.
30	39	7	0	41	57	29,98	68	0,026	W	1	Fine.
	59	2	0	59	59	30,02	50		SSW	1	Fine.

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1800	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
May 1	49	7	0	52	57	30,02	72		S	1	Cloudy.
	60	2	0	60	58	30,03	69		S	1	Cloudy.
2	53	7	0	54	58	30,05	68		E	1	Cloudy.
	67	2	0	67	61	30,05	53		SE	1	Fine.
3	49	7	0	51	59	30,07	66		E	1	Hazy.
	69	2	0	68	62	30,05	54		E	1	Fine.
4	51	7	0	55	60	30,02	70		E	1	Fine.
	75	2	0	74	64	30,01	56		W	1	Fine.
5	57	7	0	60	63	30,11	73		E	1	Fine.
	74	2	0	74	65	30,05	60		E	1	Fine.
6	50	7	0	54	63	29,97	73		NE	1	Fine.
	72	2	0	72	65	29,91	47		E	1	Fine.
7	50	7	0	54	64	29,90	67		NE	1	Fine.
	74	2	0	74	66	29,86	50		ESE	1	Fine.
8	54	7	0	59	65	29,87	62		E	1	Cloudy.
	73	2	0	72	66	29,83	53		ESE	1	Fair.
9	55	7	0	59	65	29,84	60		ENE	1	Fair.
	73	2	0	73	68	29,83	54		E	2	Fine.
10	48	7	0	50	65	29,80	80	0,120	NE	1	Cloudy.
	55	2	0	54	64	29,78	71		ENE	1	Rain.
11	48	7	0	51	63	29,78	67	0,080	NE	2	Fair.
	57	2	0	57	64	29,78	58		NE	2	Fair.
12	44	7	0	49	62	29,77	67		ENE	1	Fair.
	58	2	0	57	63	29,77	52		ENE	1	Fine.
13	46	7	0	49	62	29,78	65		NE	1	Cloudy.
	59	2	0	59	62	29,77	57		NE	1	Fair.
14	43	7	0	45	60	29,88	61		NE	2	Cloudy.
	57	2	0	57	61	29,90	56		NE	2	Fine.
15	40	7	0	44	59	30,00	59		NE	2	Cloudy.
	62	2	0	59	60	29,94	53		SE	1	Fair.
16	48	7	0	51	59	29,72	67		SE	1	Cloudy.
	57	2	0	56	59	29,58	73		SSE	2	Rain.

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1800	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
May 17	50	7	0	52	58	28,90	73	0,188	S	2	Cloudy.
	56	2	0	56	58	28,75	75		S	2	Rain.
18	48	7	0	48	58	29,38	66	0,302	WNW	2	Fine.
	60	2	0	60	60	29,50	59		WNW	2	Fair.
19	46	7	0	50	58	29,66	67	0,035	WSW	2	Cloudy.
	65	2	0	64	59	29,71	57		SW	2	Fair.
20	55	7	0	56	59	29,63	80	0,085	S	2	Rain.
	61	2	0	61	60	29,60	76		S	2	Rain.
21	49	7	0	51	59	29,78	72	0,050	S	2	Fair.
	62	2	0	62	60	29,81	56		S	2	Cloudy.
22	48	7	0	51	59	29,82	70	0,016	S	2	Fair.
	63	2	0	62	60	29,78	56		SSW	2	Fair.
23	51	7	0	53	59	29,63	70		ESE	1	Fair.
	63	2	0	62	60	29,55	65		SE	1	Cloudy.
24	52	7	0	53	60	29,65	68	0,043	S	2	Cloudy.
	65	2	0	64	61	29,75	58		S	2	Fine.
25	52	7	0	53	60	29,93	70		S	2	Cloudy.
	61	2	0	61	60	30,00	62		S	2	Cloudy.
26	49	7	0	51	59	30,20	68	0,050	SSW	1	Hazy.
	68	2	0	67	61	30,20	51		S	2	Fine.
27	52	7	0	55	60	30,12	60		S	1	Fair.
	68	2	0	67	61	30,10	61		S	1	Cloudy.
28	52	7	0	55	61	30,19	67		NE	1	Fair.
	69	2	0	69	62	30,21	50		N	1	Fair.
29	51	7	0	53	61	30,21	63		E	1	Hazy.
	66	2	0	66	62	30,16	49		E	1	Hazy.
30	52	7	0	53	61	30,05	61		WSW	1	Cloudy.
	64	2	0	64	61	29,99	58		WSW	1	Cloudy.
31	51	7	0	52	61	29,77	73	0,118	SSW	1	Rain.
	61	2	0	60	61	29,57	68		NW	1	Rain.

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1800	Six's Therm. least and greatest Heat.	Time. H. M.		Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches.	Winds. Points.	Str.	Weather.
June 1	52	7	0	53	60	29,99	70	0,232	NNE	2	Cloudy.
	58	2	0	58	61	30,07	63		NNE	2	Cloudy.
2	50	7	0	50	59	30,04	68		NNE	2	Cloudy.
	51	2	0	51	59	30,00	72		N	2	Rain.
3	50	7	0	52	58	29,86	84	0,268	NE	1	Cloudy.
	63	2	0	63	60	29,94	62		E	2	Fair.
4	53	7	0	53	60	29,98	79		NE	1	Cloudy.
	60	2	0	60	61	29,89	70		NE	1	Cloudy.
5	54	7	0	56	60	29,84	74	0,221	NE	1	Cloudy.
	66	2	0	65	61	29,80	67		NE	1	Cloudy.
6	50	7	0	50	60	29,80	77	0,085	NE	1	Cloudy.
	55	2	0	55	60	29,80	70		NE	1	Cloudy.
7	45	7	0	49	59	29,82	67		NE	1	Fair.
	62	2	0	62	60	29,80	60		SSE	1	Fair.
8	50	7	0	53	59	29,78	67		SSW	1	Cloudy.
	64	2	0	64	59	29,78	58		WSW	1	Cloudy.
9	54	7	0	55	59	29,72	65		W	1	Cloudy.
	63	2	0	63	60	29,80	49		W	1	Fair.
10	46	7	0	50	59	29,92	63		NE	1	Cloudy.
	58	2	0	58	59	29,98	53		NE	2	Fair.
11	43	7	0	46	58	30,14	64		SW	1	Fine.
	61	2	0	60	59	30,07	53		W	1	Cloudy.
12	44	7	0	47	58	30,00	62		W	1	Cloudy.
	59	2	0	59	58	30,01	57		NE	1	Cloudy.
13	46	7	0	50	57	30,00	65		WSW	1	Cloudy.
	64	2	0	64	58	29,86	58		WSW	1	Cloudy.
14	46	7	0	49	57	29,88	69	0,053	NW	1	Rain.
	57	2	0	57	58	30,00	60		NW	1	Cloudy.
15	47	7	0	50	57	30,07	63		NW	1	Cloudy.
	62	2	0	61	58	30,07	56		NW	1	Cloudy.
16	47	7	0	50	57	30,18	66		W	1	Fair.
	61	2	0	61	58	30,16	58		NW	1	Cloudy.

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1800	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter.	Rain. Inches	Winds. Points.	Str.	Weather.
June 17	50	7	0	52	58	30,19	69		NW	1	Cloudy.
	67	2	0	66	58	30,19	57		WNW	1	Cloudy.
18	52	7	0	55	58	30,18	66		WNW	1	Cloudy.
	70	2	0	70	60	30,15	58		WNW	1	Cloudy.
19	61	7	0	62	60	30,03	78	0,105	W	1	Cloudy.
	75	2	0	74	61	29,98	60		WSW	1	Cloudy.
20	61	7	0	62	62	29,84	70		WSW	2	Cloudy.
	72	2	0	72	63	29,79	56		SW	2	Fair.
21	54	7	0	56	62	29,80	67		SW	1	Cloudy.
	66	2	0	66	63	29,76	48		WNW	2	Fair.
22	49	7	0	51	61	29,81	65		WNW	1	Fine.
	65	2	0	64	62	29,85	55		NW	2	Fair.
23	48	7	0	52	61	30,02	65	0,033	SW	1	Fair.
	68	2	0	68	62	30,08	55		SW	1	Cloudy.
24	51	7	0	53	61	30,17	66		SW	1	Fair.
	72	2	0	72	63	30,19	53		SW	1	Fair.
25	52	7	0	54	61	30,18	68		SSE	2	Fine.
	73	2	0	73	63	30,08	54		SSE	2	Fair.
26	58	7	0	58	62	30,01	74		SW	1	Rain.
	68	2	0	68	63	30,06	52		WNW	1	Fair.
27	51	7	0	53	62	30,28	63		SW	1	Fair.
	72	2	0	72	64	30,27	68		SW	1	Fine.
28	57	7	0	58	63	30,34	57		E	1	Fair.
	70	2	0	69	64	30,32	50		ESE	1	Hazy.
29	58	7	0	58	63	30,28	59		SSW	1	Cloudy.
	71	2	0	69	63	30,18	59		SW	1	Cloudy.
30	55	7	0	55	63	30,19	66		NE	1	Fine.
	72	2	0	72	65	30,19	51		ENE	1	Fine.

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July 1	50	7	0	56	64	30,25	62		NE	1	Cloudy.
	67	2	0	67	65	30,22	53		NE	1	Fine.
2	54	7	0	56	64	30,18	62		SSW	1	Fine.
	75	2	0	75	66	30,11	52		SW	1	Fine.
3	57	7	0	58	65	30,00	63		WSW	1	Cloudy.
	71	2	0	71	65	29,97	57		WSW	1	Cloudy.
4	55	7	0	57	65	30,06	65		SSW	1	Fine.
	74	2	0	71	67	30,02	56		SSW	2	Fine.
5	54	7	0	56	65	30,02	65		SSW	1	Fine.
	69	2	0	69	66	30,01	55		WSW	1	Cloudy.
6	56	7	0	58	65	30,03	63		SW	2	Cloudy.
	75	2	0	75	66	30,03	53		SW	2	Fine.
7	55	7	0	58	65	30,02	66		SSW	2	Fine.
	76	2	0	76	66	30,01	56		SW	2	Cloudy.
8	62	7	0	64	66	30,06	67		SSW	2	Cloudy.
	77	2	0	77	68	30,07	59		S	2	Fair.
9	62	7	0	64	67	30,04	68		SSW	2	Fair.
	77	2	0	77	68	30,08	51		SW	1	Fair.
10	55	7	0	56	67	30,18	63		W	1	Fine.
	71	2	0	71	68	30,25	50		NW	1	Fair.
11	55	7	0	58	67	30,23	60		S	1	Fine.
	77	2	0	74	68	30,17	51		S	2	Fine.
12	62	7	0	64	68	30,15	66		W	1	Cloudy.
	76	2	0	76	69	30,12	48		W	1	Fine.
13	53	7	0	58	68	30,22	60		WNW	1	Fine.
	68	2	0	68	68	30,22	53		N	1	Cloudy.
14	56	7	0	58	66	30,23	62		E	1	Cloudy.
	66	2	0	66	66	30,23	56		SE	1	Cloudy.
15	56	7	0	57	66	30,30	59		E	1	Fine.
	71	2	0	71	67	30,27	51		E	1	Fine.
16	56	7	0	57	66	30,28	58		SW	1	Cloudy.
	80	2	0	79	68	30,26	53		N	1	Fair.

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July 17	63	7	0	67	67	30,28	65		N	1	Cloudy.
	79	2	0	79	68	30,28	57		N	1	Cloudy.
18	59	7	0	63	68	30 31	64		N	1	Cloudy.
	77	2	0	77	68	30,31	56		N	1	Fine.
19	59	7	0	62	68	[illegible]	61		N	1	Cloudy.
	75	2	0	74	69	30,20	53		N	1	Hazy.
20	56	7	0	60	68	30,21	60		N	1	Cloudy.
	73	2	0	73	69	30,19	54		W	1	Cloudy.
21	54	7	0	57	68	30,20	63		SW	1	Fine.
	76	2	0	74	69	30,15	54		SW	1	Fine.
22	60	7	0	63	68	30,11	62		NE	1	Cloudy.
	74	2	0	73	69	30,15	54		NE	1	Cloudy.
23	60	7	0	63	68	30 30	64		E	1	Hazy.
	79	2	0	79	70	30,32	53		NE	1	Cloudy.
24	64	7	0	66	69	30,40	64		NE	1	Fine.
	79	2	0	79	71	30,40	52		NE	1	Fine.
25	54	7	0	60	69	30,45	63		NE	1	Fine.
	75	2	0	75	71	30,43	55		NE	1	Fine.
26	58	7	0	60	69	30,41	64		NE	1	Cloudy.
	72	2	0	72	70	30,35	55		ENE	1	Fine.
27	54	7	0	59	69	30,26	63		NE	1	Cloudy.
	70	2	0	69	68	30,25	57		E	1	Fair.
28	52	7	0	[illegible]	67	30,21	66		E	1	Cloudy.
	72	2	0	71	68	30,21	58		NE	1	Cloudy.
29	54	7	0	60	67	30,21	64		NE	1	Cloudy.
	75	2	0	[illegible]	68	30,20	60		NNE	2	Hazy.
30	55	7	0	[illegible]	68	30,23	65		ENE	1	Fair.
	80	2	0	78	69	30,23	51		NE	1	Fine.
31	59	7	0	62	68	30,22	64		E	1	Fair.
	81	2	0	81	70	30,20	53		ESE	1	Fair.

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Aug. 1	62	7	0	63	69	30,18	63		WSW	1	Fine.
	87	2	0	85	70	30,16	50		W	1	Fine.
2	64	7	0	67	70	30,13	61		SE	1	Fine.
	89	2	0	88	72	30,18	48		SSW	1	Fine.
3	63	7	0	67	71	30,13	59		WSW	1	Fair.
	85	2	0	84	73	30,11	50		W	2	Fine.
4	63	7	0	65	71	30,05	64		W	1	Cloudy.
	75	2	0	75	73	30,07	49		NW	2	Fine.
5	54	7	0	59	70	30,23	58		W	1	Fine.
	69	2	0	68	70	30,27	51		N	1	Cloudy.
6	52	7	0	56	69	30,23	58		W	1	Fine.
	79	2	0	76	70	30,09	50		WNW	1	Fine.
7	57	7	0	61	69	30,22	59		NNE	1	Cloudy.
	72	2	0	72	70	30,28	50		NE	1	Fine.
8	54	7	0	59	68	30,37	57		W	1	Hazy.
	75	2	0	74	69	30,38	52		SW	1	Fine.
9	54	7	0	59	68	30,40	59		WSW	1	Fine.
	79	2	0	78	70	30,37	48		E	1	Fine.
10	55	7	0	63	68	30,27	62		E	1	Fine.
	79	2	0	78	71	30,18	50		E	2	Fine.
11	62	7	0	68	70	29,98	64		E	1	Fine.
	88	2	0	85	73	29,95	45		SSW	1	Fine.
12	62	7	0	66	71	30,06	41		NW	1	Fine.
	84	2	0	83	73	30,06	49		N	1	Fine.
13	58	7	0	62	71	30,19	66		E	1	Fine.
	77	2	0	77	72	30,21	55		E	1	Fine.
14	54	7	0	60	70	30,18	64		E	1	Fine.
	75	2	0	77	72	30,10	54		E	1	Fine.
15	63	7	0	65	70	30,10	60		WSW	1	Fine.
	84	2	0	83	72	30,11	48		N	1	Fine.
16	57	7	0	64	71	30,26	56		NE	1	Cloudy.
	79	2	0	78	72	30,23	47		NE	1	Fine.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Aug. 17	59	7	0	64	71	30,15	59		SW	1	Fine.
	84	2	0	82	74	30,07	46		N	1	Fine.
18	63	7	0	68	72	30,08	60		NNE	1	Fine.
	83	2	0	82	74	30,08	53		ENE	1	Fine.
19	62	7	0	64	72	29,97	72	0,041	E	1	Rain.
	82	2	0	79	73	29,91	61		SE	1	Fine.
20	65	7	0	65	73	29,88	67	0,062	W	1	Cloudy.
	80	2	0	80	73	29,82	57		SW	1	Cloudy.
21	61	7	0	62	72	29,73	66		N	2	Cloudy.
	68	2	0	68	72	29,74	54		NNW	1	Cloudy.
22	49	7	0	55	69	29,64	65	0,040	NE	2	Cloudy.
	60	2	0	55	68	29,64	69		NE	1	Rain.
23	50	7	0	54	67	29,59	75	0,410	NE	1	Fair.
	64	2	0	55	67	29,72	77		NE	1	Rain.
24	54	7	0	55	67	29,78	74	0,351	NE	2	Cloudy.
	62	2	0	58	66	29,76	76		NNE	2	Rain.
25	55	7	0	55	66	29,79	80	0,185	NE	2	Rain.
	65	2	0	65	67	29,80	72		NE	2	Cloudy.
26	55	7	0	57	66	29,91	76	0,097	NE	1	Cloudy.
	64	2	0	64	66	29,90	67		N	1	Cloudy.
27	56	7	0	57	65	29,85	77	0,085	WNW	1	Rain.
	64	2	0	62	65	29,85	76		WNW	1	Rain.
28	57	7	0	57	65	29,89	78	0,115	WSW	1	Cloudy.
	69	2	0	65	65	29,92	64		SE	1	Cloudy.
29	57	7	0	59	64	30,05	80		NE	1	Cloudy.
	69	2	0	69	66	30,06	61		NE	1	Cloudy.
30	56	7	0	59	65	30,21	82	0,080	NE	1	Cloudy.
	71	2	0	70	66	30,18	62		SE	1	Fair.
31	54	7	0	57	65	30,10	77		E	1	Cloudy.
	70	2	0	70	67	30,05	60		E	1	Fine.

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Sept. 1	53	7	0	58	65	30,06	69		NE	1	Fair.
	67	2	0	67	66	30,05	60		ENE	1	Cloudy.
2	57	7	0	60	65	29,93	78		NE	1	Cloudy.
	68	2	0	68	67	29,93	60		E	2	Fair.
3	58	7	0	60	66	29,94	77		NE	1	Cloudy.
	69	2	0	69	68	29,93	62		ENE	1	Fair.
4	54	7	0	57	67	29,89	74		E	1	Cloudy.
	73	2	0	72	68	29,82	63		E	2	Fair.
5	59	7	0	59	68	29,58	84	0,258	NE	1	Rain.
	63	2	0	59	67	29,52	79		NNW	1	Rain.
6	57	7	0	58	66	29,44	81	0,558	SSW	1	Cloudy.
	67	2	0	65	67	29,38	64		SE	1	Cloudy.
7	56	7	0	58	66	29,27	78		E	1	Cloudy.
	63	2	0	60	66	29,15	72		NE	1	Rain.
8	56	7	0	58	65	29,16	80	0,921	SSW	1	Cloudy.
	69	2	0	69	66	29,24	63		SW	2	Fair.
9	55	7	0	57	65	29,41	78		SSW	2	Cloudy.
	69	2	0	69	67	29,50	59		SW	2	Fair.
10	60	7	0	60	66	29,69	76		SW	2	Cloudy.
	67	2	0	67	66	29,85	63		NW	2	Cloudy.
11	55	7	0	56	65	30,08	67		NW	1	Cloudy.
	67	2	0	67	66	30,13	54		SW	1	Fair.
12	53	7	0	56	65	30,12	72		S	1	Fine.
	69	2	0	69	67	30,12	68		S	1	Fair.
13	59	7	0	59	66	30,24	72		NE	1	Cloudy.
	65	2	0	64	66	30,24	68		NE	1	Hazy.
14	55	7	0	56	65	30,27	75		NE	1	Cloudy.
	70	2	0	69	67	30,23	63		E	1	Fair.
15	59	7	0	61	66	30,16	85		E	1	Cloudy.
	73	2	0	72	68	30,11	64		E	1	Fine.
16	56	7	0	58	67	30,04	79		ENE	1	Cloudy.
	77	2	0	75	69	30,00	67		S	1	Fair.

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Sept. 17	59	7	0	61	68	29,99	76		S	1	Cloudy.
	71	[illegible]	0	71	69	29,93	66		SSE	1	Cloudy.
18	62	7	0	62	68	29,90	77		SSW	1	Cloudy.
	71	2	0	71	70	29,88	60		SW	1	Fair.
19	57	7	0	59	68	29,80	72		SE	2	Fair.
	66	2	0	66	68	29,76	63		S	[illegible]	Cloudy.
20	53	7	0	54	66	29,77	75	0,302	S	1	Cloudy.
	66	2	0	66	67	29,76	62		S	1	Fair.
21	55	7	0	58	66	29,50	76		S	2	Rain.
	65	[illegible]	0	65	65	29,42	73		S	2	Fair.
22	53	7	0	55	64	29,32	72	0,065	S	[illegible]	Cloudy.
	65	2	0	65	65	29,37	61		SSW	1	Fair.
23	50	7	0	54	64	29,62	72		SW	1	Cloudy.
	62	2	0	62	64	29,68	54		W	[illegible]	Fair.
24	48	7	0	53	63	29,85	68		SW	[illegible]	Cloudy.
	62	2	0	62	63	29,56	76		SW	2	Rain.
25	50	7	0	52	61	29,37	68	0,241	WSW	2	Cloudy. [Much wind last night.
	57	2	0	56	62	29,51	67		WSW	[illegible]	Cloudy.
26	50	7	0	51	61	29,94	74		N	1	Cloudy.
	60	[illegible]	0	60	62	29,96	63		NW	1	Fair.
27	52	7	0	55	62	29,75	77	0,058	SSE	2	Rain.
	62	2	0	62	62	29,62	70		S	2	Rain.
28	41	7	0	43	60	29,81	75	0,202	SW	1	Fine.
	59	2	0	59	60	29,84	63		NW	1	Cloudy.
29	49	7	0	51	60	29,72	76	0,072	S	1	Cloudy.
	62	[illegible]	0	61	61	29,59	75		S	2	Rain.
30	43	7	0	46	59	29,65	73	0,032	SW	1	Fair.
	58	2	0	58	60	29,63	60		SW	1	Cloudy.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Oct. 1	46	7	0	48	59	29,64	72		SW	1	Cloudy.
	57	2	0	57	59	29,68	66		SSW	1	Cloudy.
[illegible]	53	7	0	56	59	29,68	85	0,076	S	2	Cloudy.
	66	2	0	65	61	29,67	72		SW	2	Cloudy.
3	46	7	0	48	59	29,76	74	0,035	SSW	1	Fair.
	61	2	0	61	61	29,68	60		SSW	2	Cloudy.
4	47	7	0	48	59	29,67	71	0,098	WSW	2	Fine.
	57	2	0	56	60	29,71	62		WNW	2	Fair.
5	42	7	0	43	58	29,73	72		SSW	1	Fine.
	58	2	0	58	60	29,72	58		WNW	1	Fair.
6	49	7	0	50	58	29,49	71		E	1	Cloudy.
	56	2	0	53	59	29,34	73		E	2	Rain.
7	53	7	0	56	59	29,38	88	0,248	E	1	Cloudy.
	63	2	0	60	60	29,32	73		SSE	2	Cloudy.
8	54	7	0	54	60	29,47	75	0,097	S	[illegible]	Fair. [Much wind last night.
	62	2	0	62	61	29,52	63		S	2	Fair.
9	50	7	0	50	60	29.40	80	0,147	S	1	Fine.
	59	2	0	59	61	29,18	71		S	2	Rain.
10	47	7	0	47	59	29,03	70	0,092	S	2	Fine.
	55	2	0	54	60	29,10	64		S	2	Cloudy.
11	45	7	0	46	59	29,42	71		WNW	1	Fine.
	56	2	0	56	60	29,62	63		WNW	1	Cloudy.
12	40	7	0	41	59	29,94	68		WNW	1	Fine.
	51	2	0	51	62	30,02	58		NW	1	Fine.
13	39	7	0	44	59	30,22	73		SW	1	Fair.
	56	2	0	56	61	30,28	62		WNW	1	Cloudy.
14	45	7	0	47	59	30,30	74		SW	1	Cloudy.
	56	2	0	55	60	30,23	65		SW	1	Cloudy.
15	46	7	0	46	59	30,13	68		W	1	Fair.
	55	2	0	56	60	30,13	60		WNW	1	Fair.
16	39	7	0	40	58	30,35	72		WNW	1	Fine.
	53	2	0	53	60	30,40	60		NW	1	Cloudy.

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Oct. 17	39	7	0	41	58	30,43	72		SW	1	Hazy.
	54	2	0	54	59	30,42	64		SW	1	Hazy.
18	48	7	0	48	58	30,37	69		SW	1	Cloudy.
	56	2	0	56	59	30,36	62		WSW	1	Cloudy.
19	50	7	0	51	58	30,41	67		SSW	1	Cloudy.
	54	2	0	54	60	30,41	65		SSE	1	Cloudy.
20	50	7	0	50	58	30,37	68		SSE	1	Cloudy.
	56	2	0	56	59	30,30	66		SW	1	Cloudy.
21	43	7	0	48	58	30,05	77		SSW	1	Cloudy.
	55	2	0	54	59	29,90	78		S	2	Rain.
22	35	7	0	37	57	30,08	67	0,040	WNW	1	Fine.
	45	2	0	45	58	30,18	56		NW	2	Fine.
23	36	7	0	41	56	30,22	71		WSW	1	Cloudy.
	51	2	0	50	57	30,26	68		NW	1	Cloudy.
24	44	7	0	44	56	30,28	72		SW	1	Cloudy.
	54	2	0	53	59	30,26	60		SSW	1	Fair.
25	42	7	0	46	56	30,14	72		S	1	Cloudy.
	53	2	0	52	58	30,05	66		S	[illegible]	Cloudy.
26	46	7	0	48	56	29,66	71	0,062	S	2	Cloudy.
	55	2	0	55	59	29,55	78		SW	2	Rain.
27	36	7	0	38	55	29,71	73	0,141	S	[illegible]	Cloudy.
	49	2	0	49	57	29,75	68		SW	1	Fair.
28	44	7	0	45	56	29,86	73	0,022	WSW	1	Cloudy.
	52	2	0	52	58	29,91	68		W	1	Cloudy.
29	43	7	0	43	56	30,05	77	0,097	W	1	Fair.
	51	2	0	51	58	30,14	65		WNW	1	Fair.
30	45	7	0	46	56	30,15	65		SW	1	Cloudy.
	54	2	0	54	58	30,05	63		SSW	1	Fair.
31	48	7	0	50	57	29,90	77	0,130	S	1	Rain.
	52	2	0	52	58	29,90	63		W	1	Cloudy.

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Nov. 1	38	7	0	38	57	30,10	76		SW	1	Fine.
	52	2	0	52	58	30,13	70		SW	1	Fair.
2	45	7	0	46	57	29,86	78		S	2	Cloudy.
	53	2	0	52	58	29,70	75		S	2	Cloudy.
3	47	7	0	48	57	29,30	73	0,091	S	2	Cloudy. Much wind last night.
	52	2	0	47	59	29,30	70		WNW	2	Fair.
4	39	7	0	39	56	29.30	72	0,115	SW	1	Fair.
	47	2	0	47	57	29,30	61		SW	1	Cloudy.
5	34	7	0	34	56	29,38	69		SW	1	Fine.
	49	2	0	48	58	29,61	63		W	1	Fair.
6	41	7	0	43	56	29,72	73	0,082	E	1	Rain.
	49	2	0	49	57	29.38	84		NE	1	Rain.
7	45	7	0	45	57	29,67	84	0,323	SSW	1	Cloudy.
	54	2	0	52	58	29,49	76		SSW	1	Cloudy.
8	48	7	0	49	57	29,21	75	0,062	SSW	2	Cloudy.
	53	2	0	53	58	29,19	68		SSW	2	Cloudy.
9	49	7	0	55	57	28,82	89	1,493	S	3	Rain.
	55	2	0	47	57	28,98	77		WNW	3	Cloudy. Wind very violent. Barom fell to 28.38 this forenoon.
10	36	7	0	36	56	29,83	73	0,205	SW	1	Fine.
	56	2	0	51	58	29,73	60		SE	2	Cloudy.
11	50	7	0	54	58	29,37	86	0,210	S	2	Cloudy.
	59	2	0	59	60	29.27	85		S	2	Cloudy.
12	38	7	0	38	57	29,65	73	0,225	SSW	1	Fine.
	46	2	0	46	58	29,74	67		WNW	2	Fine.
13	34	7	0	35	55	29 96	76		WSW	1	Fine.
	45	2	0	45	58	30,03	68		W	1	Fine.
14	42	7	0	51	57	29 92	84		S	2	Rain.
	55	2	0	55	59	29,91	70		WSW	1	Fair.
15	47	7	0	51	58	29,51	79	0,065	S	2	Cloudy.
	52	2	0	52	60	29.39	68		WSW	1	Cloudy.
16	42	7	0	46	57	29.50	77		W	1	Cloudy.
	50	2	0	50	58	29.56	74		WNW	1	Cloudy.

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1800	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Nov. 17	43	7	0	43	57	29,51	73	0,037	W	1	Fair.
	47	2	0	47	58	29,56	67		NW	1	Cloudy.
18	43	7	0	43	56	29,84	73		NE	2	Fair.
	46	2	0	46	58	29,99	69		NE	2	Fair.
19	36	7	0	36	56	30,18	74		NNE	1	Fine.
	44	2	0	44	57	30,19	70		NE	1	Fine.
20	34	7	0	34	56	30,22	73		NE	1	Fine.
	44	[illegible]	0	44	57	30,36	70		NE	1	Fair.
21	38	7	0	38	56	30,27	73		SW	1	Cloudy.
	42	2	0	42	57	30,18	76		W	1	Rain.
22	34	7	0	34	54	30,18	75	0,128	W	1	Fair.
	44	2	0	44	54	30,16	73		W	1	Cloudy.
23	40	7	0	43	53	29,85	74		SSW	2	Cloudy.
	50	2	0	50	57	29.77	70		SSW	2	Cloudy.
24	46	7	0	46	54	29,59	77		SSW	2	Cloudy.
	48	2	0	48	56	29,47	78		S	2	Rain.
25	48	7	0	50	55	29,00	80	0,375	S	3	Rain. [Much wind last night.
	50	2	0	42	56	29,00	77		SW	2	Cloudy.
26	37	7	0	37	53	29,24	70	0,228	NNE	1	Cloudy.
	39	2	0	39	56	29,43	65		NW	1	Fine.
27	30	7	0	30	53	29,63	76		SSW	1	Fair.
	37	2	0	37	55	29,63	73		S	1	Fine.
28	32	7	0	33	52	29,73	73		N	1	Cloudy.
	38	[illegible]	0	38	53	29,81	75		NNE	1	Cloudy.
29	30	7	0	35	50	29,79	78	0,125	SW	1	Rain.
	42	2	0	42	53	29,89	75		N	1	Fair.
30	34	7	0	37	51	29,98	78	0,038	SW	1	Rain.
	46	2	0	46	53	29,98	74		WNW	1	Fine.

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1800	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
Dec. 1	35	8	0	46	51	30,01	83		SSW	1	Cloudy.
	48	2	0	48	53	29,98	78		SW	1	Cloudy.
2	38	8	0	40	52	29,63	79	0,480	SW	1	Cloudy.
	43	2	0	43	55	29,56	70		SSW	1	Fair.
3	33	8	0	33	51	29,41	77		W	1	Fair.
	40	2	0	40	56	29,38	73		WSW	1	Fair.
4	31	8	0	32	51	29,11	77		SSW	1	Fair.
	38	2	0	38	52	28,88	78		E	1	Rain.
5	35	8	0	35	50	28,78	80	0,175	NE	1	Foggy.
	37	2	0	37	52	28,85	78		N	1	Rain.
6	34	8	0	34	51	29,05	81	0,158	E	1	Cloudy.
	39	2	0	39	52	29,18	78		E	1	Cloudy.
7	33	8	0	34	50	29,33	80		NE	1	Cloudy.
	37	2	0	37	52	29,37	80		SW	1	Cloudy.
8	34	8	0	34	50	29,51	81		SW	1	Cloudy.
	39	2	0	39	51	29,57	78		SW	1	Cloudy.
9	33	8	0	34	49	29,60	79		ENE	1	Cloudy.
	38	2	0	38	51	29,53	75		NE	1	Cloudy.
10	34	8	0	34	48	29,65	80				Foggy.
	34	2	0	34	51	29,68	79		E	1	Cloudy.
11	33	8	0	37	49	29,77	82				Foggy.
	44	2	0	43	52	29,76	84		E	1	Foggy.
12	40	8	0	44	51	29,65	85		E	1	Rain.
	47	2	0	46	53	29,65	87		E	1	Cloudy.
13	42	8	0	42	52	29,80	87	0,067	NE	1	Foggy.
	46	2	0	46	53	29,77	87		NE	1	Cloudy.
14	44	8	0	45	52	29,65	83		E	1	Cloudy.
	49	2	0	49	54	29,70	82		E	1	Cloudy.
15	40	7	0	40	51	29,92	82		E	1	Cloudy.
	45	2	0	45	55	29,98	81		E	1	Fair.
16	37	7	0	40	52	30,18	85		ENE	1	Foggy.
	44	2	0	44	54	30,16	81		E	1	Cloudy.

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for December, 1800.

1800	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Dec. 17	37	[illegible]	0	37	53	30,11	82		E	1	Cloudy.
	38	[illegible]	0	38	52	30,10	82		E	1	Foggy.
18	37	8	0	37	52	30,10	82		E	1	Cloudy.
	37	2	0	37	53	30,10	82		E	1	Foggy.
19	35	8	0	36	52	30,03	81		SE	1	Cloudy.
	39	2	0	39	53	30,00	83		S	1	Cloudy.
20	38	8	0	47	52	29,85	88	0,035	S	1	Rain.
	51	2	0	50	54	29,85	89		SSW	1	Cloudy.
21	47	8	0	49	53	29,88	87		SW	1	Cloudy.
	51	2	0	51	56	29,94	83		SW	1	Cloudy.
22	47	8	0	48	55	29,96	83		S	1	Cloudy.
	50	2	0	49	57	29,93	78		S	1	Cloudy.
23	47	8	0	48	54	29,76	81		S	2	Cloudy.
	48	2	0	48	56	29,69	78		SSE	2	Cloudy.
24	45	8	0	48	55	29,40	80		SSE	2	Cloudy.
	49	2	0	49	58	29,40	71		SSW	2	Fair.
25	40	8	0	40	55	29,46	81	0,056	SW	1	Cloudy.
	45	2	0	44	55	29,44	76		SSW	1	Fine.
26	35	8	0	37	54	29,40	78		SW	1	Cloudy.
	39	2	0	39	57	29,40	78		WSW	1	Cloudy.
27	33	8	0	36	54	29,58	78		W	1	Cloudy.
	40	2	0	40	56	29,66	73		WNW	1	Fair.
28	33	8	0	33	52	29,76	78		W	1	Fair.
	39	2	0	39	53	29,72	75		SSW	1	Cloudy.
29	39	8	0	39	52	29,53	81	0,085	ESE	1	Rain.
	42	2	0	42	54	29,31	85		NE	1	Rain.
30	31	8	0	33	51	29,61	80	0,530	NNE	1	Snow.
	33	2	0	31	52	29,70	75		NE	1	Fair.
31	29	8	0	33	50	29,50	90	0,085	NE	2	Snow.
	34	[illegible]	0	34	53	29,90	76		NE	1	Fair.

1800.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	51	18	38,7	51	22	39,1	55	41	52,6	30,40	29,01	29,49	95	60	80,7	2,458
February	53	25	36,0	53	26	36,4	54	45	49,7	30,27	29,30	29,84	86	57	70,7	0,260
March	57	23	39,4	57	23	39,9	58	45	50,6	30,22	29,28	29,85	88	54	68,8	0,305
April	62	38	51,0	62	41	51,5	6[illegible]	56	58,1	30,02	29,27	29,68	88	50	69,3	2,885
May	75	40	57,0	74	44	57,5	68	57	61,1	30,21	28,75	29,83	80	47	63,3	1,087
June	75	43	57,8	74	46	58,8	65	58	60,3	30,34	29,72	30,00	79	48	63,0	0,997
July	81	50	65,6	81	56	66,8	71	64	67,5	30,45	29,97	30,20	68	48	58,7	0,000
August	89	50	66,4	88	54	67,4	74	64	69,4	30,40	29,59	30,05	82	41	61,4	1,466
September	77	41	60,1	75	4[illegible]	60,8	70	59	65,2	30,27	29,15	29,77	85	54	71,6	2,709
October	66	35	49,9	65	37	50,6	62	55	58,6	30,43	29,03	29,91	88	58	69,0	1,285
November	59	3[illegible]	44,1	59	30	44,3	60	5[illegible]	56,2	30,36	2[illegible],82	29,67	89	60	73,9	3,802
December	51	29	39,4	51	32	40,3	58	48	52,7	30,18	28,78	29,65	90	73	80,4	1,671
Whole year			50,5			51,1			58,5			29,90			79,2	18,925

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCI.

PART II.

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TRANSACTIONS.

XII. *A Historical and Anatomical Description of a doubtful amphibious Animal of Germany, called, by* Laurenti, *Proteus Anguinus. By* Charles Schreibers, *M. D. of* Vienna. *Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read March 26, 1801.

The singular and ambiguous animal of which I have the honour to give the following description, lives in a small lake in Carniolia, called, *Sitticher See.*

This country is celebrated for its lakes, and subterraneous caverns filled with water. One of its largest lakes is the famous lake of Czirknitz, which seems to be the source of all the others; at least there is no doubt that all of them have some connection with each other, by subterraneous, though still unexplored communications: this is ascertained by the repeated and reciprocal risings, and corresponding depressions of the water, in spring and autumn.

Notwithstanding this circumstance, the animal here spoken of

has been found only in the lake above mentioned; from which it has always appeared to have been thrown out by the rising of the water. Although these overflowings generally happen once or twice in every year, yet, notwithstanding the frequent researches of fishermen, to whom these lakes afford a good subsistance, and of other people, who have made it their particular business to search for the animal during a space of several years, very few specimens have hitherto been discovered.

Several years ago, a specimen from thence came to the Museum of Natural Curiosities in the University of Vienna; and another, nearly at the same time, came into the possession of a naturalist (Baron HOHENWARTH) in Carinthia. In the year 1795, another specimen was sent to the present Professor of Natural History at Vienna, at his particular request, by Baron ZOIS, a liberal and zealous naturalist, settled in Carniolia. I did not meet with any other specimens, during my tours in the years 1798 and 1799; though I visited most of the collections in the dominions of Austria, Germany, and a part of Italy. I know, however, that more than one came into the possession of the late Dr. SCOPOLI, who resided in Carniolia for several years.

After that time, notwithstanding frequent and diligent researches, none could be discovered till the year 1799; when I was favoured by the same naturalist with two other specimens.

The first notice of this animal was given by the late Dr. LAURENTI, (in 1768,) in his Synopsis Reptilium. The short description he gave there, was made from the specimen before mentioned, preserved in spirit by Baron HOHENWARTH, in Carinthia. LAURENTI had no opportunity to examine the internal structure; and his description of the external parts is so

defective and erroneous, that I shall only add, that he considered it as a perfect animal, and called it Proteus Anguinus.

In the year 1772, Dr. Scopoli gave a more exact and circumstantial (but not anatomical) description of this animal, in his Annus quintus Histor. Natural. p. 75.

From the accounts of these learned naturalists, and from a drawing sent by the last mentioned one, Linnæus had notice of this animal; but, not being fully acquainted with its nature and characters, and observing that it very much resembled the larvæ of some lizards, he supposed it might be an imperfect animal. It is however noticed, with some others of a similar structure, in the new edition of his Systema Naturæ, by Gmelin. Tom. i. P. 3. p. 1056.

Since that time, nothing has been done to illustrate the nature of this curious animal, and to remove the doubts of naturalists, very few of whom have taken notice of it; however, J. Hermann, (Commentarius Tabulæ Affinitatum Animalium, 1783, p. 256. note,) and the celebrated amphibiologist, T. G. Schneider, (Histor. Amphib. Fasc. 1. 1799, p. 40. &c.) think themselves fully convinced of its being an imperfect animal: they ridicule the ideas of Laurenti and Scopoli, and blame Linnæus for hesitating; though they have no sufficient foundation for their own opinion, which is formed merely from apparent analogy.

I should tire the patience of my readers, if I attempted to refute the arguments of these two naturalists, or to prove, by long deductions and comparisons, that neither they, nor the others, had sufficient ground for their conclusions; I shall therefore only give an accurate description of what I have observed in a careful anatomical dissection of it; in the per-

formance of which I have been guided by no partiality in favour of any particular opinion, but simply by the desire of knowledge; and shall not attempt to speak decisively upon the subject, until farther opportunities may afford me the means of ascertaining the nature of this interesting animal. As all attempts to send it alive to Vienna, (a distance of about 250 English miles,) have been in vain, I have had no opportunity of observing its nature and actions in the living state; I can, therefore, only communicate the few observations which Baron ZOIS had the opportunity of making, he having had the good luck to get some specimens alive, and to keep one of them so during several days. His observations were communicated to me, with the specimens, in the month of September 1795, in November 1799, and in January 1800.

In his first letter he says, he had some hopes of sending me the animal alive, as it seemed to take some nourishment, having thrown up from its stomach, the first days of his keeping it in the water of the lake, (in which it was brought to him,) a great many small shells of the genus Helix; but he soon found his hopes disappointed, as he saw that it would not take either these shells or any other food, and became from day to day more languid and weak. The seventh day it lay upon its back, and the skin became covered with a flaky glutinous slime, as is commonly observed on amphibious animals when dying. It seemed, when alive, very torpid, and moved but seldom; it swam however sometimes, with the help of its broad tail, very swiftly, in every direction. Twice he observed it touch one of those shells with the extremity of its beak, and fling it twelve inches distance, on the bottom of the vessel wherein it was kept. The first days it crept slowly on

the bottom, and seemed to look for food; it often took a shell into its mouth, but gave it out again, swallowing none. Several times it rose to the surface, stretched its head out of the water, and took in air, but returned directly to the bottom. He never could find any traces of eyes, even with a magnifying glass. He assents, however, to Dr. Scopoli's opinion, that it is an animal in a perfect state, and sui generis.

In another letter he says, that he received the two specimens sent therewith alive; but that they died in a few hours, being kept in common water.

In a third letter he says, in answer to some enquiries I made, that all the specimens he knows of were found in the months of August and September; but that some have been seen by fishermen, so early as the month of July, when the season happened to be very rainy, and the lake overflowed.

He says the animal uses its feet in creeping on the bottom, and in ascending along the sides, of the vessel, if of wood; that it creeps very slowly, or, to use his words, very deliberately. In this particular it differs from every other creeping animal, insomuch, that he is tempted to call this motion (which he says is amusing to behold) characteristic of the animal.

It often produces a hissing kind of noise, pretty loud, more so than one should expect from so small an animal, and resembling that produced by drawing the piston of a syringe. He once observed that, while doing so, it hung on the side of the vessel, with the fore part of the body out of the water. He suspects that two very small darkish spots, in a parallel direction on the forehead, might be the eyes: he discovered them by looking with a magnifier at the head, when out of the water; the animal hanging quietly on the sides, where it continued

motionless for a considerable time, without appearing shy or timid.

It is of a light red colour, when alive; and the branchial appendages, on the sides of the head, are of a deep blood colour. In spirit, it soon loses all tinge of redness. Inactivity, and the above-described singular creeping motion, seem to be peculiar to this animal; and, although it came several times in the course of a day to the surface of the water, and even rose above it, it passed the greatest part of its time at the bottom. In ascending in the water, it seemed only to make use of the tail; ascending as slowly and smoothly as it creeps. Sometimes Baron Zois observed it to use a motion like that of fishes, throwing itself about in the water with considerable force and swiftness.

The five specimens which I saw, three of which were at my disposal, were of very different sizes; the largest was about thirteen inches long, and one inch in diameter; three others were between nine and ten inches long; and the smallest (apparently the youngest and most imperfect) was about eight inches long, and hardly the third part of an inch in diameter.

Notwithstanding this considerable difference in size, which evidently shewed a difference in age, they agreed in the general construction of the external, as well as internal parts. The following description was taken from the examination and dissection of the largest specimen, and compared with those of two others of a smaller size.

The head is one inch and three quarters long; its forepart somewhat resembles the bill of a duck, being flat and narrow, rather broader behind than the body, somewhat compressed, and rendered uneven by some smooth protuberances, occasioned by strong muscles. The upper jaw is somewhat larger than the

under one, and surrounds it with a thick folded skin, forming a considerable lip, and making the mouth itself larger and broader than it would be according to the size of the skull. It has no nostrils, external ears, or appearance of eyes.

This last circumstance occasioned the mistake of Dr. Laurenti, and of all the other naturalists, who affirmed that the animal was really destitute of eyes. It is true, that there is no appearance of them after death, particularly when the animal has been kept in spirit; and indeed they are scarcely to be discovered even while it is alive; but, if the skin is removed from the front of the head, they may be seen at the base of the rostrum, beneath the foremost protuberances. They are very small, and black; seem to be very simple, and are not quite so globular as those of fishes, but more flat: they lie in a small cavity of the skull, and seemed to be somewhat attached, at least by some glutinous matter, to the skin itself; as, by removing the skin without sufficient care, they adhered to it, and came out of their cavity, along with a small thread, which I think was the optic nerve.

On the sides of the occiput are apertures, like those of fishes; and, over them, ramified branches of vessels or branchial appendages, similar to those of tadpoles or other larvæ of amphibious animals; which analogy has occasioned so many doubts and dissentions among the learned about this animal, as well as about the still ambiguous Siren lacertina of Linnæus. These appendages are formed by three very large branches of blood vessels, of which the uppermost is the largest, the next somewhat smaller, and the lowest the smallest: each of these is divided into smaller branches; which, lastly, are bordered on their under edge by many very small and thin ones. Their

direction is towards the body, almost parallel to it; and, upon removing them, the branchial apertures are seen directed to, and communicating with, the mouth, like the gills in fishes. Each of these apertures is divided by three thin simple membranes, (not vascular, as in fishes,) attached in like manner to three oblique cartilaginous bones, but leaving only two holes between them; in which circumstance, and in the red colour of the appendages during life, this animal differs materially from fishes, and from tadpoles or other larvæ.

Behind the appendages, the head becomes narrower, and forms a round neck, reaching from thence to the insertion of the fore feet, about half an inch long, and a little narrower than the body.

The body itself is round, equally thick throughout, and, from the insertion of the fore feet to that of the hind feet, about $6\frac{1}{2}$ inches long. The fore feet are about one inch long, consisting of the thigh and leg, and terminating in three toes, without nails, whereof the middle one is the longest. The hind feet are about one-sixth of an inch shorter than the fore feet; and terminate in only two ill shaped toes.

Behind these feet, the body grows narrower, and terminates in the tail, which is three inches and a half long, compressed on the sides, and very fleshy and strong in the middle; it grows narrower towards its end, which is almost pointed, and, as well as the edge above and underneath, is surrounded by a thin membrane, which gives it a considerable breadth. Underneath, rather lower than the hind feet, is the anus, an oblong aperture, surrounded by a strong wrinkled sphincter.

Upon opening the body by a longitudinal section, from the anus to the edge of the under jaw, I found the whole cavity

almost filled by the liver, which extended from the thorax down to the pelvis, so as to cover all the viscera, except the heart, the upper part of the lungs, the left half of the stomach, the gall-bladder, and the lower intestines.

The heart lies in the middle of the chest, or rather of the neck, above the insertion of the fore-feet: it is inclosed in an ample pericardium, formed by a simple thin membrane, attached to the upper part of the lungs, and to the surrounding muscles of the body.

On account of the width of the pericardium, the heart seems very large, and oval; it is, however, really small, and consists of a single ventricle, and a single auricle, as large as the ventricle, but a little flatter, finely serrated on its upper edge, and situated upon the upper part of the ventricle, a little towards the left.

I could distinguish only two considerable blood vessels in connection with the heart on its right side, where the auricle formed an angle with the ventricle. One of them, which was short, but pretty large, goes up perpendicularly, forming two enlargements, and divides, a little above the upper edge of the auricle, into two branches, which seem to take their direction towards the branchial appendages.

The other and longer vessel, which comes from the same part of the heart, below the former, goes straight down, turns in a little below the point of the heart, and forms there a considerable enlargement, just over and upon the lungs; then runs down in the middle of them, until it reaches the point of the liver, in the description of which, its farther course will be mentioned.

Besides these two vessels, (which by their size, as well as by their being filled with coagulated blood after the animal's death, seem to be veins,) I could not pursue the course of any others,

owing to the length of time the specimens had been kept in spirit.

On account of the connection of the parts, I shall proceed to describe the liver, leaving the respiratory organs at present.

The liver is the most considerable viscus of this animal, and is nearly five inches long, beginning about one inch below the heart, running down to the larger intestines, and terminating about two inches above the anus. Five lobes may be distinguished; the uppermost begins narrow and pointed, and is somewhat divided by a longitudinal ridge, just where the œsophagus terminates in the stomach.

This lobe is the longest and narrowest, is cylindrical, and runs down on the right side of the body, covering half the stomach, near the end of which it extends to the left, and forms the second lobe, which is throughout connected with the former, and with it fills up the whole cavity of this part of the belly. This second lobe terminates in a third, which lies deep in the left side, is of an oval form, with a pointed end, and has several incisions on its edges.

The fourth, a very small lobe, is formed on the under edge of the first, and only marked by two incisions, caused by the cavity in which the gall-bladder lies.

The fifth lobe is the broadest, as it fills up the whole breadth of the body: it is almost quadrangular; terminates in a point to the right, and goes off in an oblique direction to the left.

The upper surface of the liver is smooth and convex, the under one somewhat rough and concave. On the upper surface are transverse elevations like ribs; and a thin membrane runs from the pericardium, along with the blood vessel, to its beginning, and over the middle of the first lobe to the second,

fastening this part of it, like the ligamentum suspensorium in other animals, to the muscles of the body; a similar membrane connects the liver, underneath, with the parts contiguous to it.

The longer blood vessel mentioned in the description of the heart, after having formed the enlargement already spoken of, forms a double one on the upper part of the liver; then runs down on the right edge of that viscus, giving out some small branches to its substance, also one to the mesentery; and terminates upon the surface of the fifth lobe of the liver, in a great ramified branch. The colour of the liver is a dark bluish-grey, with numerous small black spots: its substance is glandulous and compact, more so than I have observed in other analogous amphibious animals, or fishes.

The gall-bladder is pretty large, so as to fill up the whole cavity formed by the lobes of the liver, to which it is, in some places, firmly attached, as it is, underneath, to the first intestine; thus having an immediate connection both with the liver and the intestines.

The œsophagus is a narrow canal, of about one inch in length: it runs down on the back; is strong, and internally full of longitudinal wrinkles, particularly as it approaches the stomach, at the orifice of which they form a perfect cardia.

The stomach is entirely distinct from the œsophagus and bowels, being infinitely wider. It forms a bag, about two inches long, of a strong and almost coriaceous appearance.* Its lower end terminates in a narrow duodenum, which continues nearly straight for about two inches, forming the small intestines, then making three considerable convolutions, and

* In the stomach of one specimen, I found the head and bones of a small fish.

terminating in the rectum, which is very strong, and wider towards the anus.

The internal surface of the stomach, towards the bowels, is very much wrinkled longitudinally, forming a perfect pylorus. These wrinkles continue throughout the whole length of the intestinal canal, and, in the large intestines, are very strong, forming serrated plicæ.

The stomach and intestines are very loose in the cavity, being connected with the neighbouring parts only by thin membranes. The rectum, however, at its termination, is firmly connected with the kidney underneath, and attached above to the ossa pubis. A little above this is a narrow viscus, about half an inch long, fastened by its lower end to the rectum, and, by a long slender membrane from its outer edge, to the muscles of the abdomen. It is hollow, of a very spongy substance, and opens into the lower end of the rectum, by means of a short narrow canal, through which I could pass a thin bristle. By its situation and structure, I suppose it to be the uterus; though I observed it in every one of the dissected specimens.

The spleen is flat and narrow, about one inch and a half long, fastened to the back of the stomach on the left, and also to the pancreas, with which it seems to be farther connected by three vessels: its substance is compact, and has a glandular appearance.

The pancreas is narrow, and about one inch long; it is attached to the back of the duodenum, and also to the gall-bladder.

The mesentery is a thin membrane, fastened strongly to the back of the intestinal canal on one side, and to the neighbouring parts on the other. It is full of blood vessels, two of which

are very conspicuous; one near the intestines, beginning from the under edge of the pancreas, and terminating on the rectum, after having sent off numerous branches to the bowels: the other is the above mentioned branch of the blood vessel of the liver; it runs on the opposite edge of the mesentery, parallel with the former, giving out numerous branches, some towards the bowels, (anastomosing with the former vessel,) others towards the neighbouring parts.

Upon the spine, and strongly fastened to it, lies a viscus, which originates in two thin membranous strings in the thorax: these strings soon unite into one; in the course of which, lower down, appears a glandulous substance, forming innumerable small and narrow convolutions, from which several blood vessels go off towards the spine. This glandulous substance, which is very thin and narrow about the middle of the body, grows sensibly larger in its course downwards, and terminates at last, a little above the beginning of the rectum, in an oblong flat viscus, of a glandulous appearance, divided in the middle by a shallow longitudinal ridge. It is a little broader than the rectum, under which it lies; it is fastened to the spine, and opens at its lower end into the rectum, by a short and narrow canal. Its situation and structure led me to suppose it to be the kidney.

There are still two other viscera, somewhat resembling the blind intestines in some fishes; they must not, however, be confounded with them, as they do not belong to the intestines, but appear evidently to be the ovaries. They are situated low in the belly, one on each side the spine, and seem to originate in a blood vessel which runs down the pneumatic bladders, (of which I am going to speak,) and a membrane connecting them

with the mesentery: these unite together, and terminate in a narrow hollow viscus, about half an inch long, and impervious at its broader end. Its substance seemed to be composed of small glands. In one of the specimens, it had the appearance of a transparent membranous bag, containing a mass of small glands, or little eggs, of the size of a grain of millet, conglutinated together.

Below the heart, in the thorax, there is a bag about one inch long, of a very simple and thin membrane, without any apparent vessel. The upper end of this bag is round and impervious: the lower end terminates in two ducts, of which the right, accompanied by a blood vessel, runs down under the liver, and is connected to its outer edge by a membrane, until it reaches the middle of the body, where it grows wider, and terminates in a small oval membranous bladder. In one specimen, this bladder was very small, and semilunar. The blood vessel accompanying the duct goes as far as the middle of this bladder, then separates from it, joining with the membrane of the mesentery; thus forming the beginning of the ovary on this side, after having received some small branches of the vessel coming from the liver to the mesentery. The bladder itself terminates in a sharp point. The duct on the left side runs down like the former, but under the stomach, to which it is fastened by a membrane: it runs lower down in the belly than that on the right, and terminates in an oblong bladder, of the same shape and structure as the former, but always considerably larger.

The blood vessel which comes to the bladder along with its duct, divides into two branches: one runs over the surface of the bladder, is very large, and divides into numerous branches

which seem to anastomose with the branches of a smaller vessel, probably an artery, running on the opposite side. The other branch goes, in the manner before described, to the ovary of the left side, which is likewise connected by a membrane with the mesentery.

Upon opening the pneumatic bag above mentioned, I found it quite simple, without any cellular structure, as in other amphibious animals; but there was an intermediate membrane, which separated it into two cavities, between which, however, a communication was left, at the upper end of the bag, by a large semilunar opening. The communication of these cavities, with their respective ducts and bladders, was plainly shewn by blowing air into them, which readily passed from one to the other: indeed the ducts, though very narrow, admitted water to be driven through them by means of a fine syringe, so that I was able to fill both bladders entirely. In the back of the upper part of the bag there is a small opening, which terminates, by a very narrow canal or trachea, in a small slit or glottis, at the bottom of the lower jaw; shewing very evidently, that these parts constitute the respiratory organs or lungs of this animal.

I shall not attempt to construct any hypothesis respecting the nature and manner of living of this animal, or make any deductions respecting the singular structure of these parts, so much deviating from the common organization. I shall only remark, that Mr. SCHNEIDER, in his work already quoted, mentions, though obscurely, and only in a few words, (p. 63, &c.) that he observed a similar structure of the respiratory organs, in what he supposes a larva of the Lacerta palustris. It seems extraordinary that he paid so little attention to

such a surprising organization. I have dissected many larvæ of water lizards, but never could discover any such structure.

The skin of this animal is very tenacious and coriaceous: by looking at it with a magnifier, there appears a quantity of minute glands under the epidermis, similar to those in water lizards, &c. It is strongly attached to the muscles underneath, in several places, by the cellular membrane, in which the above mentioned glands are dispersed, and which is filled, particularly upon the back, with a tough viscous matter. Of the muscular fibres, I observed three different layers, which however are very thin, but firmly connected with each other: the fibres of the two outer layers are very tender, and transverse; those of the middle layer are stronger, and longitudinal. On the under side of the lower jaw, are the following strata. Immediately under the skin is a thin stratum of transverse fibres; under this, in the middle, is a stratum of longitudinal fibres; there are also two very large and strong muscles on each side, of which the upper ones consist of fibres running obliquely from the middle outwards and downwards: the under ones are somewhat stronger, and consist also of oblique fibres, not running parallel to the first, but still more obliquely, and almost transverse. The forehead and the occiput are covered by five muscles; two in the middle of the forehead, very large, and composed of strong transverse curved fibres, divided only by a ridge from each other, and forming together the figure of a heart, with the point backwards: their fore part forms the declivity of the front to the rostrum, (which is only covered by a thin layer of longitudinal fibres,) and the two tubercules on the forehead, which Dr. SCOPOLI took for eyes. On each side of these muscles there is another muscle, composed of very

strong longitudinal fibres, narrow on the forehead, but very large on the occiput: in the middle of this there is a thin narrow stratum of straight longitudinal fibres, covering the occiput itself, and extending from the point of the heart-shaped muscle, over the neck and spine. By these very strong muscles, the head appears of a large size, and the rostrum is very conspicuous, which otherwise, according to the form of the bones, would be very flat, narrow, and almost cylindrical.

The skin is in some places firmly connected with the muscles of the head; but its strongest connection is on the outer edge of the rostrum, with the bone itself, (where it forms, by a duplicature, a large thick lip,) and on the sides of the occiput, where it forms the branchial appendages; these are a continuation of the epidermis, forming, or at least investing, blood vessels attached by a duplicature of the skin to the sides of the head. On the back, besides the cover of transverse fibres, is a stratum of strong longitudinal ones.

There is, in each jaw, a row of very minute sharp teeth.

The tongue is pretty large and fleshy: it is loose at the point, but attached by its root to the lower jaw, and fastened on both sides, by two muscular strings, to the os hyoides.

In the pharynx, above the œsophagus, there is a very small oblong slit, or glottis, (like that which, in fishes, leads to the swimming bladder by means of a canal,) without epiglottis; but, as it is situated between the longitudinal fibres at the beginning of the œsophagus, it contracts and shuts, when the œsophagus is longitudinally extended in swallowing the victuals.

On each side of the lower jaw appear the three branchial cartilages, to which, as is above mentioned, the membranes are attached.

The bones seem to be of the same conformation and nature as in salamanders. I could not construct a perfect skeleton; as the flesh was so strongly contracted and hardened by the spirit, that I was obliged to boil it some time, by which the bones were dissolved. However, I can assert, that there were no ribs, or sternum; but there were bones in the tail.

Though I shall not enter into a comparison of this animal with those which are analogous to it,* I must say something respecting its analogy with the famous Siren lacertina of LINNÆUS; with which it agrees in the most striking particulars, viz. in having gills and lungs, and therefore causes the same doubts about its being a perfect animal. The doubts respecting the Siren are not yet removed; but are rather increased, by different anatomical accounts, given by two equally renowned anatomists, HUNTER and CAMPER. In conformity to the opinion of the latter, who asserted that the animal was destitute of lungs, it has lately been removed from the class of amphibious animals, and transferred to that of fishes, under the name of Muræna Siren. The principal difference between the Siren and the animal here described, (besides the former having only two feet,) consists in the head and lungs. In the Siren, the head is short, having no rostrum, but a pointed small mouth, conspicuous eyes, and nostrils. The lungs, although constructed also of a simple membrane, without cellular subdivisions, and running down the whole length of the body on each side, are divided

* By these I mean, the larvæ of Salamanders and of some water lizards, the Proteus Tritoneus of LAURENTI and SOELMANN, and the animal lately (but very imperfectly) described in the Transactions of the American Philosophical Society for the year 1799, as a new species of Siren. The latter, in shape, in size, and in the form and structure of the head and feet, is totally different from the animal described in this Paper, being much more analogous to the above mentioned larvæ.

from the beginning, and are throughout equally wide; neither forming the ducts, nor the remarkable bladders, described in this Paper. Notwithstanding this material difference, and many others of less consequence, there is no doubt that these two animals are nearly allied to each other.

Having thus given a faithful account of this ambiguous animal, and as perfect a one as circumstances enable me to do, I shall only express my hopes, that it will be considered as an interesting addition to the knowledge of natural history and comparative anatomy, whether the animal here described be supposed a perfect one, or whether it be considered as the larva of some unknown species. To those who may incline to the latter opinion, I shall only say, that notwithstanding the most careful researches during many years, and the frequent fishing in the lakes and caverns of the neighbouring country, at every season of the year, no animal has hitherto been detected, of which it can possibly be the larva.

EXPLANATION OF THE PLATES.

PLATE XVI.

The animal, apparently in its full grown state, in the act of swimming.

PLATE XVII.

Fig. 1. *The liver.*

a, Its beginning, with a rostriform point.

b, b, The first, and uppermost, narrow cylindrical lobe.

c, The second lobe, to the left, a continuation of the former.

d, d, The third lobe, a continuation of the second, quite in the left side.

e, The fourth lobe.

f, f, The fifth and largest lobe.

g, The connection of this lobe with the upper part of the liver, to the right.

h, Termination of the liver on the right side.

i, A cavity formed by the different lobes, for the reception of the gall-bladder.

k, A varicose dilatation of the blood vessel running from the heart to the liver.

l, Course of this blood vessel, on the outer edge of the liver, to the right.

m, Its double dilatation, on the upper corner of the liver.

n, n, Its farther course downwards.

o, o, Its branches going to the superficies of the liver.

p, Its termination, in a ramified branch, on the superficies of the fifth lobe.

q, *q*, *q*, A membrane originating from the pericardium, running longitudinally over the middle of the liver, as far as the third lobe, to the left, and attaching the liver to the muscles of the belly.

Fig. 2. *The intestines and organs of respiration.*

a, *a*, The pericardium laid open, the heart being removed.

b, The upper part of the air-bag, forming the beginning of the lungs.

c, *c*, The bag itself, formed by a thin and simple membrane, and divided into two cavities.

d, *d*, The right pneumatic duct, being a continuation of the right cavity of the air-bag.

e, The air-bladder in which this duct terminates.

f, *f*, A thin membrane, connecting the pneumatic duct with the outer edge of the liver, to the right.

g, *g*, The left pneumatic duct, running underneath the stomach, to the air-bladder.

h, The œsophagus.

i, *i*, The stomach.

k, The termination of the ventricle in the duodenum.

l, *l*, The small intestines.

m, *m*, *m*, The large convoluted intestines.

n, The rectum.

o, The anus.

p, A viscus attached to the rectum, probably the uterus.

q, *q*, A membrane connecting this viscus with the muscles of the belly.

r, A part of the spleen.

s, The gall-bladder.

t, t, The two ends of the pancreas.

u, u, The mesentery.

v, v, A blood vessel originating from the lower end of the pancreas, running down the mesentery, and sending branches to the intestines.

w, w, A branch of the blood vessel of the liver, running parallel to the former, with which it anastomoses.

x, Another branch, anastomosing with the blood vessel of the right pneumatic duct, and running to the ovary of that side.

y, y, The blood vessel of the right pneumatic duct, terminating in the ovary.

z, The supposed right ovary.

Fig. 3. *The same intestines and organs, on the opposite side.*

a, a, The œsophagus.

b, b, The stomach.

c, c, The small intestines.

d, d, d, The large intestines.

e, e, The rectum.

f, The viscus attached to it.

g, g, The spleen, attached to the stomach on the left.

h, h, The pancreas, attached to the duodenum.

i, The gall-bladder, attached on one side to the pancreas and the duodenum.

k, k, k, The mesentery.

l, l, The pancreatic blood vessel.

m, m, The hepatic blood vessel.

n, The right ovary.

o, The left ovary.

p, *p*, The air bag.

q, *q*, The left pneumatic duct, attached to the stomach by a membrane, and terminating in the air-bladder.

r, *r*, The left air-bladder.

s, *s*, The blood vessel accompanying this duct, and terminating upon the air-bladder in a large ramified branch.

t, *t*, The right pneumatic duct, terminating in its air-bladder.

u, The right air-bladder.

v, *v*, A membrane connecting the duct with the liver.

Fig. 4. *The viscus supposed to be the kidney.*

a, *a*, Its beginning in two membranous strings, in the thorax, upon the spine.

b, *b*, Its course while yet merely membranous.

c, *c*, The same, increasing gradually in size and substance, and being very much convoluted.

d, *d*, A membrane accompanying it, and attaching it to the spine.

e, *e*, *e*, Branches of blood vessels running from it, across the membrane, to the spine.

f, *f*, Termination of this viscus, under the rectum, in a flat, oblong, spongy body, divided superficially, by a longitudinal ridge, into two equal parts.

Fig. 5. *The head and upper jaw, with the skin removed.*

a, The bill-like prolongation of the forehead, covered by a thin stratum of longitudinal muscular fibres.

b, The thick upper lip.

c, c, The eyes.

d, d, The large muscles of the forehead, forming together the figure of a heart.

e, e, The strong lateral or temporal muscles, composed of oblique fibres.

f, The occipital muscle, composed of longitudinal fibres.

Fig. 6. *The inside of the under jaw.*

a, The under lip.

b, b, A row of teeth.

c, The tongue.

d, d, The muscles of the tongue.

e, e, The muscles of the jaw.

f, The pharynx.

g, The small aperture of the trachea, or rima glottidis.

h, The beginning of the œsophagus.

i, i, i, The three branchial cartilages.

Basire sc.

Fig. 3.

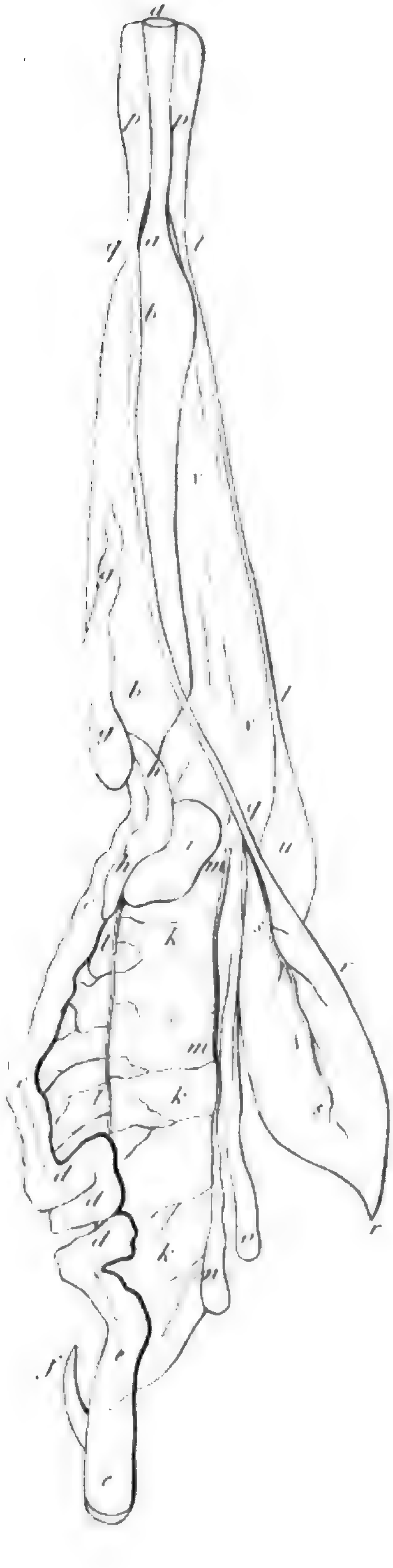

Fig. 4.

Fig. 5.

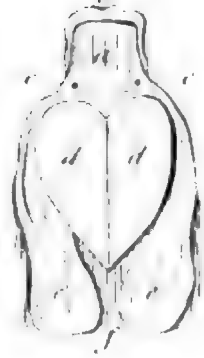

Fig. 6.

XIII. *Observations tending to investigate the Nature of the Sun, in order to find the Causes or Symptoms of its variable Emission of Light and Heat; with Remarks on the Use that may possibly be drawn from Solar Observations. By* William Herschel, *L. L. D. F. R. S.*

Read April 16, 1801.

On a former occasion I have shewn, that we have great reason to look upon the sun as a most magnificent habitable globe; and, from the observations which will be related in this Paper, it will now be seen, that all the arguments we have used before are not only confirmed, but that we are encouraged to go a considerable step farther, in the investigation of the physical and planetary construction of the sun. The influence of this eminent body, on the globe we inhabit, is so great, and so widely diffused, that it becomes almost a duty for us to study the operations which are carried on upon the solar surface. Since light and heat are so essential to our well-being, it must certainly be right for us to look into the source from whence they are derived, in order to see whether some material advantage may not be drawn from a thorough acquaintance with the causes from which they originate.

A similar motive engaged the Egyptians formerly to study and watch the motions of the Nile; and to construct instruments for measuring its rise with accuracy. They knew very well, that it was not in their power to add a single inch to the

flowing waters of that wonderful river; and so, in the case of the sun's influence, we are likewise fully aware, that we shall never be able to occasion the least alteration in the operations which are carried on in the solar atmosphere. But, if the Egyptians could avail themselves of the indications of a good Nilometer, what should hinder us from drawing as profitable consequences from solar observations? We are not only in possession of photometers and thermometers, by which we can measure from time to time the light and heat actually received from the sun, but have more especially telescopes, that may lead us to a discovery of the causes which dispose the sun to emit more or less copiously the rays which occasion either of them. And, if we should even fail in this respect, we may at least succeed in becoming acquainted with certain symptoms or indications, from which some judgment might be formed of the temperature of the seasons we are likely to have.

Perhaps our confidence in solar observations made with this view, might not exceed that which we now place on the indications of a good barometer, with regard to rain or fair weather; but, even then, a probability of a hot summer, or its contrary, would always be of greater consequence than the expectation of a few fair or rainy days.

It will be easily perceived, that in order to obtain such an intimate knowledge of the sun as that which is required for the purpose here pointed out, a true information must be first procured of all the phenomena that usually appear on its surface. I have therefore attended to many circumstances, that have either not been noticed at all before, or have not been examined with any particular view of information. The improvement also in the solar apparatus of my ten-feet telescope, by which I can

take away as much light and heat as required, has given me additional facility of making a great number of particular observations; and, as they have been all directed to an investigation of certain points, I shall give them here, in the order which the arrangement of my subject will require.

It will be necessary, before I can enter into a detail of the observations, to give notice that, from an improved knowledge of the physical construction of the sun, I have found it convenient to lay aside the old names of *spots*, *nuclei*, *penumbræ*, *faculæ*, and *luculi*, which can only be looked upon as figurative expressions that may lead to error. Nor were these few terms sufficient to describe the more minute appearances on the sun, which I have to point out.

The expressions I have used are *openings*, *shallows*, *ridges*, *nodules*, *corrugations*, *indentations*, and *pores*. It will not be amiss to give a short explanation of these terms.

Openings are those places where, by the accidental removal of the luminous clouds of the sun, its own solid body may be seen; and this not being lucid, the openings through which we see it may, by a common telescope, be mistaken for mere black spots, or their nuclei.

Shallows are extensive and level depressions of the luminous solar clouds, generally surrounding the openings to a considerable distance. As they are less luminous than the rest of the sun, they seem to have some distant, though very imperfect, resemblance to penumbræ; which might occasion their having been called so formerly.

Ridges are bright elevations of luminous matter, extended in rows of an irregular arrangement.

Nodules are also bright elevations of luminous matter, but

confined to a small space. These nodules, and ridges, on account of their being brighter than the general surface of the sun, and also differing a little from it in colour, have been called faculæ, and luculi.

Corrugations, I call that very particular and remarkable unevenness, ruggedness, or asperity, which is peculiar to the luminous solar clouds, and extends all over the surface of the globe of the sun. As the depressed parts of the corrugations are less luminous than the elevated ones, the disk of the sun has an appearance which may be called mottled.

Indentations are the depressed or low parts of the corrugations; they also extend over the whole surface of the luminous solar clouds.

Pores are very small holes or openings, about the middle of the indentations.

Any other terms I may hereafter use, will be sufficiently explained by the observations in which they occur.

I shall now enter into an examination of all the phenomena that may be observed in viewing the sun through a good telescope, beginning with those that are most common; a critical investigation of which will lead us gradually to such as are more intricate.

It will be seen that I have brought my observations under a number of short heads, or propositions, such as my subject requires. The advantage of this method is, that the tendency of every observation will be immediately understood, while it is read; whereas, had I arranged these observations in the order in which they were made, the mixture of the various points to be ascertained by them must have brought on a considerable obscurity; and, in drawing conclusions from them afterwards,

a repetition of the observations which were to support them would have been unavoidable.

I must take notice of what will perhaps be censured in many of the observations; they may be said to be accompanied with surmises, suppositions, or hypotheses which should have been kept separate. In defence of this seeming impropriety, I must say, that the observations are of such a nature, that I found it impossible, at the very time of seeing the new objects that presented themselves to my view, to refrain from ideas that would obtrude themselves. It may even be said, that since observations are made with no other view than to draw such conclusions from them as may instruct us in the nature of the things we see, there cannot be a more proper time for entertaining surmises than when the object itself is in view.

Now, since the suggestions that have been inserted were always such as arose at the moment of the observations, they are so blended with them, that they would lose much of their value as arguments, if they were given separately.

In order not to lengthen this Paper unnecessarily, I have given but a few observations under each head; especially with those propositions which may be looked upon as already sufficiently established by the observations of other astronomers. The whole tenor of the observations I have given, though divided under such numerous heads, is indeed such as must produce a mutual support; so that, frequently, one or two particular observations were thought sufficient to establish my point, when I might have added many more, from my journals, in support of it.

OF OPENINGS.

Openings are Places where the luminous Clouds of the Sun are removed.

That those appearances which have been called spots in the sun, are real openings in the luminous clouds of the solar atmosphere, may be concluded from the following observations, where the sides or thicknesses of the borders which limit the openings are distinctly described.

Jan. 4, 1801. There is a large opening much past the centre of the sun, with a shallow about it.* On the preceding side, I see the thickness of the shallow, from its surface downwards. On the following side, I also see the edge of the shallow near the opening; but it is sharp, and its thickness cannot be seen.

I see also the side of the elevation surrounding the shallow, going curvedly down to the surface of the shallow, on its preceding side.

A large collection of openings, of very different sizes, are near the following limb. They all manifest the same kind of optical appearance, but on the side which is contrary to that in the opening before mentioned; for here the thicknesses or depths of the shallows, and of the slopes going down from the upper surface to the shallows, are only visible on the following side, but not on the preceding one.†

* See Plate XVIII. Fig. 1 and 2.

† For a geometrical proof of the depression of the nucleus of a *spot*, as an opening was formerly called, see a most valuable paper of Observations on the Solar Spots, by the late ALEXANDER WILSON, M. D. Professor of Practical Astronomy in the University of Glasgow. Phil. Trans. Vol. LXIV. Part I.

Large Openings have generally Shallows about them.

Jan. 24, 1801. The two largest openings of January 19, are completely surrounded by shallows.

Many Openings are without Shallows.

Jan. 22, 1800. There are two openings which have not the least shallow about them. The corrugations go equally between and around them.

Feb. 7, 1800. There are two considerable openings not far from each other: they have no shallows about them.

Small Openings are generally without Shallows.

Dec. 2, 1800. There are a great number of large and small openings: the large ones have shallows about them; the small ones are mostly without.

Openings have generally Ridges and Nodules about them.

Dec. 20, 1794. There are two openings near the preceding margin of the sun; they have elevated extensive luminous ridges about them.

Dec. 23, 1799. A pretty considerable opening, on the following side of the disk, is surrounded by many ridges.

Feb. 7, 1800. Following two considerable openings, which are not far from each other, are several irregularly dispersed ridges, more bright than the rest of the sun.

Jan, 4, 1801. Many clustering openings are lately come into the disk; a crowd of ridges and nodules surround, and are interspersed among them.

Openings have a Tendency to run into each other.

Dec. 25, 1799. The large opening of December 23, and the small one near it, are now nearly run into each other.

Jan. 4, 1801. The two largest openings of Jan. 2, are nearly joined into one.

Jan. 6, 1801. The largest of the preceding openings, of a set observed Jan. 4, has drawn together all the small ones, and is increased in dimensions.

Jan. 29, 1801. $2^h\ 10'$. A longish opening, observed at 12^h, is increased by the addition of two projections. With more attention, however, I perceive that these projections are united to each other, but separated from the longish opening, by a narrow luminous bridge, or compressed row of luminous clouds.

Jan. 30, 1801. The two united projections of yesterday are now joined to the longish opening.

New Openings break out near other Openings.

Dec. 23, 1799. There is a small opening near the large one observed yesterday, which then was not visible.

Jan. 21, 1800. The preceding of two openings observed before, has now two other small ones near it.

Probable Cause of Openings.

Jan. 18, 1801. Between two clusters of openings, near each other, there are some, as I suspect, incipient openings: they resemble coarse pores of indentations.

Jan. 19, 1801. The incipient openings, between the clusters of yesterday's observation, are completely turned into considerable openings. It seems as if an elastic, but not luminous gas,

had come up through the pores or incipient openings, and spread itself on the luminous clouds, forcing them out of the way, and widening its passage.

Feb. 18, 1801, $7^h\ 44'$. The south preceding one of three large connected openings,* has a narrow branch coming from its shallow.

$9^h\ 55'$. The opening is broken out at three places;† and the shallow has three projections just opposite. It is plain that the breakings-out and the projections must have the same cause; which probably acts first at the opening, and widens it, then goes forward, and occasions the corresponding projections in the shallows.

The shallow is very large on that side where the breakings-out are situated; and, on the contrary, very narrow on the opposite, or, as it may be called, the quiescent side.

$10^h\ 12'$. The broadest of the little sprouting shallows is opposite the broadest of the breakings-out, or encroachments of the opening on the general shallow.

Direction and Operation of the disturbing Cause.

Jan. 24, 1801. A small oblong opening, near a preceding large one, has on its north side a very long shallow. This made me surmise, that the elastic fluid coming out of it might have a strong direction from below, towards that side. Examining therefore this opening all round, I found that the shallow extended only to one side, leaving the other parts full of luminous matter close to the margin of the opening. And, on examining the large opening, I found that the shallow about it was also larger, in the same direction as the shallow about the small one

* See Plate XVIII. Fig. 3. † See Fig. 4.

Eight other small openings, forming together a cluster with the former two, have every one also their incipient shallows on the same side; that is, towards the north-following; and none at all on the other parts of their margins.

$3^h\ 15'$. The shallow about the small opening has changed its direction. It was like Fig. 5, and is now as in Fig. 6. (Plate XVIII.) In the farthest end of it, I expect an opening to be coming on.

$4^h\ 30'$. There is already the appearance of an incipient opening.*

Jan. 6, 1801. The shallows about the large opening and the small one near it, are much altered in situation and dimensions. All the smaller openings near them have undergone great changes; some being gone, and new ones come in different places, while others have altered their appearance. The bias in the direction of the shallows is also considerably changed.

Jan. 30, 1801. $9^h\ 20'$. There is a cluster of small openings, which I expect to see united into one, by the breaking down, driving away, or dissolution, of the intermediate communications of luminous clouds. From having seen it yesterday, I know the largest of the openings to be a generating one. It has an increasing shallow; and the next largest opening of the cluster has an incipient one.

$10^h\ 40'$. The incipient shallow is now pretty large.

$11^h\ 2'$. There is now also an incipient shallow about the most south of the small openings.

$11^h\ 6'$. The shallow of the largest opening is increased.

Maxima of Openings.

Jan. 29, 1801, $11^h\ 0'$. The shallow of the largest of many

* See Plate XVIII, Fig. 7.

openings that are visible, is most extended towards the north-following side.* It affects a circular form more than the opening, but is not concentric with it. It has a small lip on the north, which I suppose denotes the direction of the gas coming out of the opening. A similar lip is visible in the opening itself, as if the gas, in coming out, pressed against the luminous clouds which limit the opening, and belong to the flat.

2^h 10′. The lip or projection of the shallow about the opening is filled up at the sides; they being now as broad as the lip's projection. The filling up is marked with points in the figure.

Jan. 30, 1801. The large opening observed yesterday is no longer increased; but seems to be nearly at its maximum.

Feb. 4, 1801, 1^h 10′ The shallow of the large opening is much more round than the opening, though not concentric with it. Hence, its figure being no longer disturbed, I guess that the opening is near its maximum.

There is some Difference in the Colour of Openings.

March 1, 1800. There are two large openings, which seem to be partially covered, or rather to have a thin, semi-transparent, luminous veil of clouds still hovering over them; this gives them a fainter black colour than openings generally have.

Openings divide when they are decaying.

Dec. 26, 1799. An opening observed the 25th is reduced; and is divided in the middle by a lucid line.†

Dec. 27, 1799. The luminous bridge or passage across the opening is pretty broad, and has a branch about the middle. This branch of light has the appearance of a luminous cloud,

* See Plate XVIII. Fig. 8. † See Fig. 9.

irregularly breaking out from the passage, towards the southern half of the opening.

Dec. 28, 1799. The opening is so completely vanished, that I cannot find the least mark of its former existence. From the appearance of the branch yesterday, in the luminous division resembling a bridge thrown over a cave, I surmise that this branch, as well as the sides of what I call the bridge, have extended themselves, and as it were drawn a lucid curtain over the opaque surface of the sun.

Decaying Openings sometimes increase again.

Feb. 8, 1801. The great opening, which has been gradually diminishing since Feb. 4, is now enlarged again.

When Openings are divided, they grow less, and vanish.

Dec. 29, 1799. The south preceding of two openings observed yesterday is divided into three small ones.

Dec. 31, 1799. The divided opening of the 29th exists no longer.

Feb. 9, 1800. Of two considerable openings observed Feb. 7, the preceding one is divided into several smaller parts.

Feb. 10, 1800. The divided opening observed yesterday is in a vanishing state.

Feb. 11, 1800. The openings are all covered in, and no trace of them can be found.

Decayed Openings sometimes become large Indentations.

Feb. 9, 1800. The preceding of two openings observed the 7th of February is now about half filled up; and that half contains two indentations, with black pores, or rather remaining

small openings. They are nearly of the size of the general corrugations of the solar surface at present.

Decaying Openings turn sometimes into Pores.

Feb. 10, 1800. $1^h\ 0'$. Between the half of the opening and that part which was nearly covered in yesterday, is a set of indentations, with pores rather larger than they are in general.

When Openings are vanished, they leave Disturbance behind.

Dec. 28, 1799. $12^h\ 10'$. There is a place among the corrugations, where they are coarser now than they were an hour ago.

1^h. Two considerable openings are broken out, in the place where the corrugations were coarse. They are both so completely dark, and free from thin luminous clouds, that it appears very plainly they were only hidden behind the slight covering of luminous obstructions; one of them is about the place where the opening of yesterday was situated.

Jan. 24, 1800. There is a pretty large place, which contained the openings observed the 22d, the luminous clouds of which are in a state of disturbance: it includes five or six places, where the pores of indentations assume the shapes of incipient openings, and after some time lose them again, more or less.

Feb. 11, 1800. There is a place which I suppose to be that where the now vanished openings were yesterday: it seems to be rather disturbed in the arrangement of its corrugations. One of the indentations is probably an evanescent opening, as it still shews a considerable pore.

Apparent View into the Openings, under luminous Bridges and Shallows.

Dec. 27, 1799. A luminous passage across a large opening has all the appearance of a bridge going over a hollow space; and I have no doubt but that it is at a considerable distance from the opaque surface of the sun.

Jan. 4, 1801. A ridge which separates two openings much past the centre, shews its thickness on the following side: it has a shallow, the preceding side of which I see going down to the opening; and it appears to me, that there is a considerable distance between the lowest part of that shallow and the dark surface of the sun under it. Nor can I help believing that I see aslant under it, towards the preceding side; though I find it difficult to account for such vision, on the principles of solar perspective.

Jan. 6, 1801. The same opening is now further advanced towards the limb of the sun. It appears again to me, that on the preceding side I see far under the shallow. I suspect the part towards P to be of a deeper dark colour than that towards F;* but the difference, if there be any, is not marked enough to be decisive.

Depth of the Openings, indicated by their Darkness.

Jan. 15, 1801. $11^h\ 10'$. One of the openings, which is near the preceding limb of the sun, is remarkably black. The tint of openings may perhaps assist us to infer their depth; which must be greater in a direction towards us, when the opening is near the circumference, than when it is near the centre, if the

* See Plate XVIII. Fig. 1 and 2.

distance from the shallows to the opaque surface should be considerable.

I have compared the darkness of two openings near the centre, with the appearance of that near the preceding limb. It is decidedly in favour of the blackness of the latter: this is however larger than the former two; which may occasion deceptions.

The opening near the limb is certainly darker than the two which are near the centre.

Feb. 5, 1801. An opening very near the preceding margin is of a deep black colour; certainly more so than another which is not far off, but is more towards the centre of the sun.

Feb. 8, 1801. The large opening near the margin, is darker than two other openings which are about the centre of the solar disk: the difference, however, is hardly sufficient to be decisive. The two openings are also much smaller; which may occasion a deception.

Distance between the Shallows and solar Surface, indicated by the free Motion of low Clouds.

Jan. 25, 1801. $9^h\ 22'$. A large opening, which I have been observing since the 19th, is now much advanced towards the limb.* I can see into it; and, on the preceding side, as it appears to me, a good way under the lowest regions of the clouds of which the shallow consists. The upper margin of the shallow is very sharply determined; but the clouds of the lower part of it, on the contrary, are more dispersed; some of them hanging a good way down, towards the surface of the sun's body.†

$10^h\ 20'$. The preceding side of the shallow of the large

* See Plate XIX. Fig. 10. † See Fig. 11.

opening, is now more abruptly terminated at the bottom of its thickness; the hanging or projecting clouds being removed towards the following side.*

OF SHALLOWS.

Shallows are depressed below the general Surface of the Sun; and are Places where the luminous solar Clouds of the upper Regions are removed.

That those appearances which have been called penumbræ are real depressions, or shallows, may already be concluded, from what has been related with regard to the slopes from the upper surface of the sun, down to the top of these shallows; and will follow still more evidently, from an observation of one of them on the very limb of the sun.

Dec. 3, 1800. There is a considerable opening just come into the disk, which is followed by another that is actually in the limb of the sun. The uniformity of the circular termination of the limb suffers a small deviation; it being somewhat depressed, owing to the shallow about the opening. I do not yet perceive the opening itself with certainty; but suppose it will appear to be one, when it advances more into the disk.

The Thickness of the Shallows is visible.

Jan. 6, 1801. There is a large opening much advanced beyond the centre, with a shallow round it. On the preceding side of the shallow, its descent down towards the opening is visible; but, on the following side, I see abruptly into the opening.

* See Plate XIX. Fig. 12.

Sometimes there are Shallows without Openings in them.

Feb. 7, 1801. There is a pretty large shallow inclosed by the ridges which follow some preceding openings.

Feb. 12, 1800. A place where yesterday I saw five or more nodules, at present contains low ridges inclosing some shallows.

Incipient Shallows come from the Openings, or branch out from Shallows already formed, and go forwards.

Jan. 18, 1801. In a cluster of openings, there is an incipient shallow, coming from one of them.

Jan. 19, 1801. The incipient shallow is increased, and has now spread all round the opening.

Jan. 24, 1801. A large opening sends from its shallow already formed, a narrow projection, towards the end of a neighbouring shallow belonging to a smaller opening, as if they were going to meet.

Probable Cause of Shallows.

Jan. 25, 1801, 9^h 20′. Two branches A B,* of a shallow coming from an opening C, are going towards the south. It seems as if they were destined to meet the incipient shallow of a south-following opening D.

9^h 50′. The shallow B is now very nearly united to the narrow part of the shallow surrounding the opening D. The shallow A seems to advance, in a direction towards the farthest south-following opening E.

10^h 20′. The shallow B is now completely run into the shallow about D; † and the shallow A is grown broader towards F.

11^h 30′. The shallow B is so completely joined to the shallow

* See Plate XIX. Fig. 13. † Fig. 14.

about D, that it appears as if it had not come from the opening C. The shallow A now ends in a sharp point.*

$12^h\ 50'$. The shape of the shallow A is again much changed; it is no longer pointed, but very broad at the end; † and there is a new branch breaking out at G. These changes seem all to denote, that the shallows are occasioned by something coming out of the openings, which, by its propelling motion, drives away the luminous clouds from the place where it meets with the least resistance; or which, by its nature, dissolves them as it comes up to them. If it be an elastic gas, its levity must be such as to make it ascend through the inferior region of the solar clouds, and diffuse itself among the superior luminous matter.

$1^h\ 10'$. The new branch G increases; and the openings C, D, E, are enlarged. A new branch is also breaking out from the shallow about E. It is marked H in Fig. 14, and denoted with points. These changes seem to prove, that the same gas which diffuses itself over the shallows has forced open the passages at first, and is now widening them. Hence, the increase of the openings is an additional circumstance which points out the cause of the shallows.

$1^h\ 20'$. From the shallow of a very large preceding opening, which is in an increasing state, are lately projected three small branches *a, b, c.* ‡

$2^h\ 30'$. The vacancies between the three small projecting shallows are now filled up by the same cause that occasioned them, so as to have given them the shape of an uniform but broader shallow, on the side where the branches come out, as denoted by points.

* See Plate XIX. Fig. 15. † Fig. 16. ‡ Fig. 17.

Shallows have no Corrugations, but are tufted.

Feb. 4, 1801. The great shallow about a large opening has no corrugations.

Feb. 18, 1801. The lower clouds of the shallow of a large opening, though not corrugated, are not smooth, but tufted. They are so closely connected in their tufts, that it makes them appear as if, in every vacancy, there were clouds under clouds, that prevent our looking far into them.

Decay of Shallows.

Jan. 30, 1801. The borders of the shallow belonging to the large opening observed Jan. 29, seem to be remarkably high; so that, if the opening were near the limb, they would probably appear like ridges. The shallow has again a lip, nearly in the place where there was one yesterday. But it seems as if the lip, which is visible now, had a cause contrary to what produced it yesterday. For the luminous clouds all round the shallow seem no longer to be kept off by an issuing elastic fluid, but are probably now breaking in upon the shallow, except in the place of the lip, where some energy, like that exerted yesterday, may still remain in action; in that case, the shallow, as well as the opening, is past its maximum.

OF RIDGES.

Ridges are Elevations above the general Surface of the luminous Clouds of the Sun.

Dec. 27, 1799. On the south-following side of the sun's disk, close to the margin, are some bright ridges. They are all in a

direction parallel, or nearly parallel, to the margin; and have the appearance of elevations.

Jan. 29, 1801. Two sets of openings, near the north-following limb, have wide-spreading ridges about them: four other sets, being farther advanced into the disk, do not shew any. This denotes them to be thin elevations, which can only be seen near the circumference, by a side view.

Feb. 8, 1801. Many ridges and nodules are now to be seen about the large opening, which yesterday had none. I suppose they are become visible by its advancement towards the margin, to which it draws near.

Length of a Ridge.

Dec. 27, 1799. I measured one of the longest ridges in view. It extended over an angular space of 2′, 45″,9, which is nearly 75000 miles.

Ridges generally accompany Openings.

Feb. 5, 1801. Three sets of openings near the preceding limb, and two near the following one, are surrounded by luminous ridges.

Ridges are also often in Places where there are no Openings.

Dec. 22, 1799. On the following side of the sun are luminous ridges; but not within 50 or 60 degrees of an opening.

Jan. 4, 1801. Towards the north, near the limb, is a collection of ridges without openings.

Feb. 5. Two of the sets of openings of yesterday are gone; but have left extensive ridges behind.

Ridges disperse very soon.

Dec. 28, 1799. The appearance of the ridges I saw yesterday is much changed: they are less luminous and extensive than they were. The range is much broken; and they appear more in detached irregular elevations.

Dec. 29, 1799. The ridges are so much reduced to the resemblance of the rest of the sun, that had I not known where to look for them, I should hardly have been able to trace any vestige of them.

Feb. 9, 1800. The ridges which followed some openings Feb. 7, exist no longer.

Different Causes of Ridges hinted at.

Jan. 4, 1801. A crowd of ridges and nodules surround, and are interspersed among, a cluster of openings. A ridge which crosses one of the openings like a bar or bridge, is sharp on the following side, but shews thickness on the preceding. It seems probable, that the openings permit a transparent elastic fluid to come out, which disturbs the luminous matter on the top, so as to occasion ridges and nodules. There are not less than 17 openings in the cluster.

Jan. 6, 1801. Following a set of openings lately come into the disk, are many luminous and broad, that is to say, high ridges, without any openings among them; so that the cause which produces them acts probably below the shining matter. Their own levity also may occasion them to go into the higher regions.

Jan. 30, 1801. Near the following margin is an extended plane, full of bright ridges and nodules, with a great number of

openings lately broken out, and still breaking out among them. This leads us to suppose that some elastic gas, acting below the luminous clouds, lifts them up, or increases them; and at last forces itself a passage through them, by throwing them aside.

OF NODULES.

Nodules are small, but highly elevated, luminous Places.

Jan. 24, 1800. On the south, near the limb of the sun, is a nodule; and on the south-following side is another, with two smaller ones near it. They are round, or roundish, bright elevations, of the same nature as the ridges.

Feb. 19, 1800. There are two small bright nodules, on the preceding limb of the sun. Why they should only be seen near the sun's margin, can only be explained by admitting their elevation.

Nodules may be Ridges foreshortened.

Dec. 27, 1799. Mixed with many ridges, nearly parallel to the margin of the sun, are here and there small thicknesses or knotty places. I take them to be ridges in a more central direction, which gives them the shape of nodules.

The most north of three nodules, in one of the ridges, seems to be highest in elevation. If it should last till to-morrow, it will then appear whether that nodule is really more elevated than the rest, or whether it is the foreshortening of a ridge extended in the direction of a radius.

OF CORRUGATIONS.

Corrugations consist of Elevations and Depressions.

Dec. 23, 1799. The corrugations have a mottled appearance. I see the figure of the dark and bright places. Many of the dark places are not round, but a little extended in different directions, and appear to be lower than the bright places. This, if admitted, will explain why the corrugations towards the margin of the sun, cannot so readily be seen as about the middle of the disk.

Jan. 4, 1800. The day being very favourable, I saw the sun uncommonly well. The corrugated surface presented its elevations and depressions, with as much distinctness as the rough surface of the moon.

Corrugations extend all over the Surface of the Sun.

Dec. 23, 1799. The corrugations extend all over the sun. They are less distinct all around towards the limb, than at the centre.

Jan. 22, 1800. I followed the corrugations from the centre to the circumference; and could trace them every where to within, I suppose, two minutes of the margin.

Jan. 24, 1800. The corrugations are equally spread over the whole surface of the sun. I viewed them distinctly in every part of it; and traced them with much attention to within, I suppose, half a minute of the margin.

Jan. 4, 1801. The corrugations are extended all over the disk of the sun, and go to the polar regions, as well as to the equatorial parts.

Dispersed Ridges or Nodules make Corrugations.

Nov. 17, 1800. The surface of the sun appears richly filled with very small broken or dispersed ridges, which produce the corrugated appearance.

Feb. 18, 1801. The high parts of the corrugations contain numberless separations, like small nodules, which leave room for the indentations to be seen between them.

Corrugations change their Shape and Situation; they increase, diminish, divide, and vanish quickly.

Dec. 27, 1800. $1^h\ 0'$. There is a pretty large corrugation near a small opening, which serves me as a direction to find the place. Its indentation is about four diameters of the opening from it; and 10 or 16 degrees north-preceding.

$1^h\ 5'$. I have seen the corrugation again; and find its indentation larger than it was, and farther from the opening.

$1^h\ 10'$. It is vanished; and several other such very minute changes have taken place.

$1^h\ 12'$. Within a diameter of the opening, and a little north-following it, is an oval indentation, nearly as large as the small opening.

$1^h\ 15'$. Its shape is altered; and it is divided into a corrugation, with two indentations.

$1^h\ 35'$. Both are entirely gone.

Jan. 18, 1801. Between two clusters of openings, that are near each other, there are some incipient openings, which resemble coarse corrugations, and establish a step between small openings and pores of indentations. I shewed them to my friend Dr. WILSON, who happened to be upon a visit to me,

at Slough. He saw the same phenomena; and judged of their being a link in the chain of appearances, as I did.

We drew a small sketch of the place of the phenomena, merely to serve us to communicate our observations to each other.*

1^h 19′. A, is a small opening, without a shallow, which we had fixed upon, by way of enabling us to find again the minute objects which were to be examined. B, is the indentation, or dark place, of a corrugation I pointed out to Dr. WILSON. C, is a dark place of a corrugation he pointed out to me.

1^h 24′. We both found the dark part of the corrugation B gone; and C had either changed its place or was vanished.

1^h 34′. C, was certainly gone.

Dr. WILSON pointed out another round pore, which we had not perceived before, at some distance; also a largish indentation near the opening, which guided our research. Shortly after, we found the indentation gone; and the pore was further removed from the opening.

OF INDENTATIONS.

The dark Places of Corrugations are Indentations.

Dec. 27, 1799. That the low places of the corrugations are not much depressed, is evident from their visibility pretty near the margin of the sun.

Jan. 27, 1800. The corrugations in many places are so coarse, that their indentations resemble small shallows. The

* See Plate XIX. Fig. 18.

indentations go down at the sides, like circular arches, presenting their concavities to us; but the bottom of them is nearly flat.*

Indentations are without Openings.

Jan. 15, 1801. The low places of corrugations do not contain punctures; but seem to be irregularly shaped places, of less luminous matter than the borders which inclose them.

In some Places the Indentations contain small Openings.

Dec. 27, 1799. On examining some of the largest corrugations with a high magnifying power, I see plainly, that the less bright parts, or indentations, are small openings; and that those dark places, which were the coarsest, shew the opaque surface of the sun best: some of them are as black as the large openings.

The Elevations and Indentations of Corrugations are of different Figures.

Feb. 18, 1800. Among all the corrugations, I could hardly perceive any that were round: they were of all shapes; chiefly lengthened.

Indentations change to Openings.

Feb. 10, 1800. Three corrugations, observed an hour ago, are now so enlarged, that their indentations are passed over from their former state, to small openings.

* See Plate XIX. Fig. 19.

Indentations are of the same Nature as Shallows.

Jan. 30, 1801. The depressed parts, or indentations, of corrugations, are of the colour of shallows; and are probably of the same depth below their elevations, as shallows are below the general surface of the sun.

Indentations are low Places, which often contain very small Openings.

Jan. 2. 1801. That indentations are small hollow places, and that the pores in them are little openings, may be concluded from a set of real openings of different sizes, of which I see no less than 13. Four of them are visible openings; five of them are less than the smallest openings, and larger than the indentations of corrugations; the remaining four may already be called large pores. We cannot expect to see into these pores, as we do into holes, their diameter being too small.

Indentations are of different Sizes.

Jan. 31, 1800. The indentations are very uniform, but not round. It seems they admit of every possible shape.

Indentations are extended all over the Sun.

Dec. 20, 1794. I can follow the indentations, from the centre up to the margin of the sun; but it requires great attention, as, on account of the sphericity of the disk, they become gradually less conspicuous, the nearer we go to the circumference. I saw them equally well at the north pole of the sun.

Dec. 22, 1799. The whole disk of the sun is strongly indented.

With low magnifying Powers, Indentations will appear like Points.

Feb. 4, 1800. I tried a magnifying power of only 45 times, and the sun then appeared punctulated, instead of indented. The points, or rather darker coloured places in the punctulations, were of different figures; few of them being round.

OF PORES.

The low Places of Indentations are Pores.

Dec. 20, 1794. The lowest parts of the indented appearances, are almost dark and depressed enough to be called very minute openings.

Pores increase sometimes, and become Openings.

Feb. 10, 1800. Two indentations, observed an hour ago, are increased, and contain large pores, as if they were going to be converted into visible openings, like the indentations of three neighbouring corrugations mentioned under a former article.

Jan. 22, 1800. Between some large and small openings were two pores, that grew darker while I looked at them, and may now be taken for very small openings. This seems to trace the openings to their origin, and perfectly connects the pores with them.

Pores vanish quickly.

Dec. 27, 1800. A small pore, that was north-following a very small opening, which served me as a direction for finding it again, $1^h\ 30'$ ago, is no longer to be seen.

$12^h\ 30'$. There were two pores north-preceding the same opening. When I returned to the telescope, in order to describe their situation exactly, they were vanished.

OF THE REGIONS OF SOLAR CLOUDS.

It must be sufficiently evident, from what we have shewn of the nature of openings, shallows, ridges, nodules, corrugations, indentations, and pores, that these phenomena could not appear, if the shining matter of the sun were a liquid; since, by the laws of hydrostatics, the openings, shallows, indentations, and pores, would instantly be filled up; nor could ridges and nodules preserve their elevation for a single moment. Whereas, many openings have been known to last for a whole revolution of the sun; and extensive elevations have remained supported for several days. Much less can it be an elastic fluid of an atmospheric nature: this would be still more ready to fill up the low places, and to expand itself to a level at the top. It remains, therefore, only for us to admit this shining matter to exist in the manner of empyreal, luminous, or phosphoric clouds, residing in the higher regions of the solar atmosphere. The following observations, will explain and support this idea more at large.

Changes in the Solar Clouds happen continually.

Feb. 19, 1800. In order to find whether the solar clouds were subject to very quick changes, I fixed my attention on several places; but, on looking off, even for a moment, the spots I had marked for the purpose could not be found again.*

* See what has been said of the quick changes among the corrugations and indentations, under the former articles.

There are two different Regions of Solar Clouds.

Feb. 19, 1800. It is not possible to see the sun more distinctly than I do at present. The corrugations are evidently caused by a double stratum of clouds; the lower whereof, or that which is next to the sun, consists of clouds less bright than those which compose the upper stratum. The lower clouds are also more closely connected; while the upper ones are chiefly detached from each other, and permit us to see every where through them.

Feb. 5, 1801. An opening near the preceding limb has no shallow about it. I can see the thickness of the preceding partition, from the top of the luminous clouds down to the vacancy; and perceive that the lower part of the descent is of a less bright nature than the upper one: it is of the colour of an incipient shallow.

The inferior Clouds are opaque, and probably not unlike those of our Planet.

Feb. 5, 1801. The shallows about three considerable openings, on the following side of the sun, are of the same colour with that of a large opening on the preceding side.

Feb. 18, 1801. The tufts of the shallows, or lower clouds, are all of the same colour.

The shallows about the three largest openings are all of the same colour, which is that of all the shallows I have ever seen.

Feb. 4, 1801. The colour of a very small shallow about a little opening, is as faint as that of a large shallow of a very large opening now in view; and, as far as I can remember, all the shallows I have seen have been nearly of the same colour.

Hence we have reason to conclude, that there are two different regions of clouds, the lowest whereof is never affected in colour by the cause which acts upon the upper one, when shallows are generated. If so, these clouds are probably of a very different nature; for, were they not different, they would not be differently affected by the same cause. Perhaps this lower region is a set of dense opaque planetary clouds, like those upon our globe. In that case, their light is only the uniform reflection of the surrounding superior self-luminous region. If this be admitted, it will at once account for the sameness of the colour of the shallows, and of their tufts; and for many other phenomena.

Quantity of Light reflected from the inferior Planetary Clouds.

Feb. 7, 1801. I made an artificial contrivance, for the use of my photometer, to represent a portion of the bright surface of the sun, and of an opening with a shallow surrounding it. The opening was represented by a small patch of black velvet, resembling nearly, in shape, the large opening in the sun which is now visible. This was fastened on the farthest vane of my photometer, covered with white paper, and arranged so as to be in the line of the centre. In the nearest vane, covered with the same paper, and equally prepared so as to move in the centre of the photometer, I cut a hole large enough to shew the black velvet on the farthest vane, with a small margin of its paper about it, which was intended to represent the shallow about the opening. The illumination of the nearest vane was to represent the brightness of the sun. The two vanes were arranged so as to be one behind the other, in a straight line;

and a single hole was made in the middle of the moveable piece No. 4, of the photometer described in my last paper.*

I now viewed the opening and shallow in the sun, and immediately after went to the photometer, to examine the artificial phenomenon. By withdrawing the farthest vane, I diminished its illumination, till I found the small visible rim about the velvet less luminous than the paper of the first vane, in the same proportion as I judged the shallow to be less bright than the rest of the sun. Then, going alternately many times to the telescope and to the photometer, and making such little alterations in the apparatus as I thought necessary, I obtained at last a result, which shewed that the rim of paper representing the shallow reflected 469 parts of the incident light.

Hence, if the superior self-luminous clouds of the sun throw the same quantity of light on the inferior region of opaque solar clouds as they send to us, it follows, that those inferior clouds of which the shallows are composed, reflect about 469 rays out of every thousand they receive.

With regard to the solar surface which we see in the openings, I also found that black worsted, which, by my lately published tables, reflects 16 rays out of a thousand, was not dark enough, compared to the blackness of the opening, but that black velvet seemed to be nearly of the same intensity; so that, probably, when the luminous surface of the sun sends us 1000, and the flats 469 rays, the solid surface seen in the openings reflects no more than about seven.

* See Phil. Trans. for 1800. p. 528.

Indentations are planetary Clouds, reflecting Light through the open Parts of the Corrugations.

Jan. 15, 1801. The corrugations do not contain pores, but irregularly shaped depressed places, of a darker or rather less luminous matter than the borders of the corrugations; probably owing to the same cause that makes the shallows appear less bright than the general surface.

Feb. 7, 1801. The corrugations go up to the borders of the shallow of an opening observed since Feb. 4. Indeed the high parts of the corrugations themselves consist of the upper self-luminous clouds; while their indentations are the reflection of light from the lower regions of the opaque ones.

The opaque inferior Clouds probably suffer but little of the Light of the self-luminous superior Clouds to come to the Body of the Sun.

Feb. 5, 1801. The shallow of a large opening, though already contracted, is still sufficiently broad not to permit a single direct ray of the superior self-luminous clouds to enter this opening; and the blackness of the opening shews, that but little light can penetrate through the inferior region of planetary clouds of which the shallow consists.

Motion of the inferior Clouds.

Feb. 6, 1801. The great opening of Feb. 4, is much diminished: it is now divided by a branch of the inferior clouds of the shallow, with a few superior ones upon the following half of the division or bridge. The shallow on the other side of the bridge is plainly still free from self-luminous clouds.

$11^h 53'$. On the preceding side, more of the planetary clouds are advancing to draw themselves over the opening; they are very faint.

Feb. 7, 1801. Another passage is now thrown over the great opening, consisting evidently of the lower clouds. Perhaps a few clouds of the upper regions may be drawn upon it; but, at both sides, the shallow continues to be visible from the bridge to its margin, or confinement by the surrounding self-luminous clouds.

$10^h 40'$. A third passage is beginning to come on from the following side, also consisting of lower clouds. It seems that the curtain will be closely drawn over the opening, before many of the self-luminous clouds can advance.

Motion of the superior Clouds.

Feb. 5, 1801. The large opening of Feb. 4, is in a diminishing state. Its shallow is contracted; and, though it has no corrugations, it seems as if a few self-luminous clouds of the upper regions were here and there scattered over it. I see their superior brightness, and their elevation above the shallow in the places where they are.

$1^h 42'$. More of the clouds of the upper regions have scattered themselves over the shallow; and one of them faintly passes over part of the opening, almost across it. This will probably form a division.

$1^h 50'$. There is an opposite cloud advancing to meet the former one; probably that part of the opening where they are, opposes less resistance than the rest.

$1^h 53'$. The shallow contracts very fast; the sides of the upper regions of clouds press on; the borders become irregular,

and jagged, according to the advancement of the clouds; but a coarsely circular form of the shallow in general is still preserved.

Eminent Use of the planetary Clouds.

It has been shewn that the openings, compared to the luminous surface of the sun, reflect less than 16 rays out of a thousand, and probably not more than seven. To account for this extraordinary darkness, it must be remembered, that according to the observations which have been given, hardly any but transmitted rays can ever come to the body of the sun. The shallows about large openings are generally of such a size, as hardly to permit any direct illumination from the superior clouds to pass over them into the openings; and the great height and closeness of the sides of small ones, though not often guarded by shallows, must also have nearly the same effect. By this it appears, that the planetary clouds are indeed a most effectual curtain, to keep the brightness of the superior regions from the body of the sun.

Another advantage arising from the planetary clouds of the sun, is of no less importance to the whole solar system. We have shewn that corrugations are every where dispersed over the sun; and that their indentations may be called shallows in miniature. From this we may conclude, that the immense curtain of the planetary solar clouds is every where closely drawn; and, as our photometrical experiments have proved that these clouds reflect no less than 469 rays out of a thousand, it is evident that they must add a most capital support to the splendour of the sun, by throwing back so great a share of the brightness coming to them from the illumination of the whole superior regions.

OF THE SOLAR ATMOSPHERE.

The Sun has a planetary Atmosphere.

Our observations on the double regions of clouds in the sun, are certainly a sufficient proof of the existence of a solar atmosphere. The clouds of the lower regions of the sun bear such a resemblance to our own, that they can only, like ours, be upheld by a thin elastic medium, in which, like ours in air, they may freely move about in all directions.

The Sun's planetary Atmosphere extends to a great Height.

If we have concluded, from the appearance of the clouds of the lower regions, that they were supported by an atmosphere, the same will hold good with regard to the self-luminous clouds of the upper regions. For, though probably they do not swim or float in the planetary atmosphere of the sun, like the lower ones, it is evident, from observation, that they arrange themselves regularly at certain given altitudes; which can only be ascribed to the specific gravity of the gases to which they owe their existence. Besides, as the solar atmosphere is elastic, it can be no otherwise confined than by its gravitation to the sun, in the same manner as the air, by its own weight, is kept down to the earth; and the solar atmosphere must therefore expand itself considerably above the highest ridges and nodules.

The planetary Atmosphere of the Sun is of a great Density.

This may be deduced directly from the known quantity of matter in the sun. Sir ISAAC NEWTON has proved, that the gravitation of bodies on the surface of the sun, is 27 times stronger

than it is with us. Hence, the compression of the elastic gases of which the solar atmosphere consists, if similar to our own, must be greater than that of ours, in proportion to the superior force by which they are compressed, namely, their own powerful gravitation towards the sun.

The Solar Atmosphere, like ours, is subject to Agitations, such as with us are occasioned by Winds.

A proof of this may be drawn from the observations which have been given. In several instances, we have seen the planetary clouds move over the openings; which could not have happened, unless the atmosphere in which they floated had been considerably agitated. In many other instances, we have shewn that a strong bias existed in the direction of the cause which generates the shallows. This indeed is so evident, that I have hardly ever seen a single shallow which had not some excentricity; the smallest segments of these shallows being always turned towards what I have called the quiescent side of the openings. To this may be added, that the continual luminous decompositions in the superior regions, and the consequent necessary regeneration of the atmospheric gases that serve to carry them on, and which probably are produced below the inferior cloudy regions, near the surface of the sun's body, must unavoidably be attended with great agitations, such as with us might even be called hurricanes.

There is some clear Atmospheric Space, between the solid Body of the Sun and the lowest Region of the Clouds.

From what has just now been said of the agitations which appear to take place in the solar atmosphere, it follows, that

those biases which have been shewn to affect the direction of a number of shallows at the same time, must have arisen from a motion of the atmospheric gases under the clouds; and, that there is a considerable vacant space between these clouds and the solid body of the sun, is also to be inferred, from the free motion of clouds considerably lower than usual, which were seen to pass over an opening, and which cannot be supposed to have rolled over the ground in contact with it. Without, however, entering into any particular examination of the amount of the distance from the sun to the first cloudy regions, which, were I to guess by some pretty obvious circumstances, would not be less than some hundreds of miles, we may take it for granted, that the altitude of the clouds will every where be determined by their own density, and by that of the atmosphere in which they are suspended.

The Sun's planetary Atmosphere is transparent.

It will be easily shewn, that the gases of the solar atmosphere are transparent; for we have already given observations that prove our being able to see the reflected light of the corrugations from their indentations; and of the self-luminous regions in general, from the shallows which they surround and illuminate. To this may be added, that we also see clearly down through the space which leads through the openings, as fully appears from our being able to see the thicknesses of the borders which inclose them. We have likewise given an instance of seeing the limb of the sun broken by a vacancy proceeding from a large shallow; though undoubtedly that shallow must have been covered with the solar atmospheric gases.

THEORETICAL EXPLANATION OF THE SOLAR PHENOMENA.

We have admitted, in order to explain the generation of shallows, that a transparent elastic gas comes up through the openings, by forcing itself a passage through the planetary clouds. Our observations seemed naturally to lead to this supposition, or rather to prove it; for, in tracing the shallows to their origin, it has been shewn, that they always begin from the openings, and go forwards. We have also seen, that in one case, a particular bias given to incipient shallows, lengthened a number of them out in one certain direction, which evidently denoted a propelling force acting the same way in them all. I am, however, well prepared to distinguish between facts observed, and the consequences that in reasoning upon them we may draw from them; and it will be easy to separate them, if that should hereafter be required.

If however, it be now allowed, that the cause we have assigned may be the true one, it will then appear, that the operations which are carried on in the atmosphere of the sun are very simple and uniform.

Generation of Pores.

By the nature and construction of the sun, an elastic gas, which may be called empyreal, is constantly formed. This ascends every where, by a specific gravity less than that of the general solar atmospheric gas contained in the lower regions. When it goes up in moderate quantities, it makes itself small passages among the lower regions of clouds: these we have frequently observed, and have called them pores. We have shewn that they are liable to continual and quick changes, which must be a natural consequence of their fleeting generation.

Formation of Corrugations.

When this empyreal gas has reached the higher regions of the sun's atmosphere, it mixes with other gases, which, from their specific gravity, have their residence there, and occasions decompositions which produce the appearance of corrugations. It has been shewn, that the elevated parts of the corrugations are small self-luminous nodules, or broken ridges; and I have used the name of self-luminous clouds, as a general expression for all phenomena of the sun, in what shape soever they may appear, that shine by their own light. These terms do not exactly convey the idea affixed to them; but those of meteors, coruscations, inflammations, luminous wisps, or others, which I might have selected, would have been liable to still greater objections. It is true, that when speaking of clouds, we generally conceive something too gross, and even too permanent, to permit us to apply that expression properly to luminous decompositions, which cannot float or swim in air, as we are used to see our planetary clouds do. But it should be remembered, that, on account of the great compression arising from the force of gravity, all the elastic solar gases must be much condensed; and that, consequently, phenomena in the sun's atmosphere, which in ours would be mere transitory coruscations, such as those of the aurora borealis, will be so compressed as to become much more efficacious and permanent.

Cause of Indentations.

The great light occasioned by the brilliant superior regions, must scatter itself on the tops of the inferior planetary clouds, and, on account of their great density, bring on a very vivid reflection. Between the interstices of the elevated parts of the

corrugations, or self-luminous clouds, which, according to the observations that have been given, are not closely connected, the light reflected from the lower clouds will be plainly visible, and, being considerably less intense than the direct illumination from the upper regions, will occasion that faint appearance which we have called indentations.

Cause of the mottled Appearance of the Sun.

This mixture of the light reflected from the indentations and that which is emitted directly from the higher parts of the corrugations, unless very attentively examined by a superior telescope, will only have the resemblance of a mottled surface.

Formation of small Openings, Ridges, and Nodules.

When a quantity of empyreal gas, more than what produces only pores in ascending, is formed, it will make itself small openings; or, meeting perhaps with some resistance in passing upwards, it may exert its action in the production of ridges and nodules.

Production of large Openings and Shallows.

Lastly, if still further an uncommon quantity of this gas should be formed, it will burst through the planetary regions of clouds, and thus will produce great openings; then, spreading itself above them, it will occasion large shallows, and, mixing afterwards gradually with other superior gases, it will promote the increase, and assist in the maintenance, of the general luminous phenomena.

If this account of the solar appearances should be well founded, we shall have no difficulty in ascertaining the actual state of the sun, with regard to its energy in giving light and heat to our globe; and nothing will now remain, but to decide the question which will naturally occur, whether there be actually any considerable difference in the quantity of light and heat

emitted from the sun at different times. But, since experience has already convinced us, that our seasons are sometimes very severe, and at other times very mild, it remains only to be considered, whether we should ascribe this difference immediately to a more or less copious emission of the solar beams. Now, as we have lately had seasons of deficiency, that seem to indicate a want of the vivifying principles of light and heat, and as, from the appearance of last summer, and the present mild winter, there seems to be a change that may be in our favour, it will be proper to have recourse to solar observations, in order to compare the phenomena which indicate the state of the sun, with the seasons of these remarkable times. The following two sets, which are selected from my journals, I believe will assist us materially in this inquiry.

SIGNS OF SCARCITY OF LUMINOUS MATTER IN THE SUN.

Visible Deficiency of empyreal Clouds.

July 5, 1795. $1^h\ 6'$. The appearance of the sun is very different from what I have ever seen it before. There is not a single opening in the whole disk; there are no ridges or nodules; there are no corrugations.

A perfect Calm in the upper Regions of solar Clouds.

Dec 9, 1798. $12^h\ 33'$. The sun has no openings of any kind; nor can I perceive any places that look disturbed, like those where openings have lately been.

Want of Openings, Ridges, and Nodules.

Sept. 18, 1795. There is no opening in the sun. I viewed it with powers from 90 to 460.

April 1, 1798. $11^h\ 49'$. I examined the sun with a power of 230; but could find no openings.

Nov. 27, 1799. The sun is without openings. I cannot however perceive any indication that, by the mere look, would denote a deficiency of light.

Dec. 31, 1799. There are no openings in the disk of the sun.

Jan. 3. 1800. There is no opening visible any where.

Jan. 27, 1800. There is no opening. There are no ridges; nor is there a single nodule any where.

Jan. 30, 1800. There are neither openings, ridges, nor nodules, in the sun.

Jan 31, 1800. There are neither openings, nor ridges, in the sun.

Feb. 4, 1800. There are neither openings, ridges, nor nodules, any where.

Feb. 11, 1800. There is not an opening, ridge, or nodule, any where in the sun.

Feb. 18, 1800. There are no openings, no ridges, or nodules.

Dec. 22, 1799. In one part of the sun are some vivid ridges; but I cannot find any of them in other parts.

Dec. 27, 1799. Near the following margin are some bright ridges; but there is not a single one to be seen in any other part of the sun's disk.

Many Indentations without, and others with, changeable Pores.

Jan. 3, 1800. The indentations contain fewer black points than last week.

Jan. 4, 1800. The corrugations are punctured with blackish indentations. The sun is more affected in this manner than it was yesterday.

Jan. 27, 1800. The sun is every where coarsely indented, but not punctured; there being no black points in the indentations.

SIGNS OF ABUNDANCE OF LUMINOUS MATTER IN THE SUN.

Visible Increase of empyreal Clouds.

Feb. 12, 1800. The indentations, in many parts, are changed to small shallows of corrugations. There seems to have been a gradual increase of the luminous clouds for some time past. The reason why I am not positively assured of this increase is, that my present method of viewing the sun is so much better than formerly, that, by seeing things to greater advantage, there may be some deception in the seeming change of appearances.

March 5, 1800. I can now entertain no doubt that the luminous clouds are more copious than they were some time ago. The corrugations seem all to be better filled. Hardly any of the indentations have pores.

Many Openings, Ridges, and Nodules.

March 5, 1800. A range of openings has a very fine appearance; there are 55 of them. The most south and largest has a considerable shallow about it. Two, just north of it, have shallows on the northern side, but not towards the south, where the borders of the openings seem to be full as elevated as the highest luminous clouds. Near the south-following margin are extensive ridges, studded here and there with nodules.

Nov. 17, 1800. The sun is beautifully ornamented with openings, shallows, ridges, and nodules.

Dec. 2, 1800. The sun is every where richly covered by luminous clouds. Ridges and nodules are also to be seen in many places.

Dec. 27. A large opening is lately come into the disk; several other small ones are visible; and there are, near the preceding and following limbs, many extensive ridges. The luminous clouds are very plentifully and richly scattered all over.

Jan. 15, 1800. There are three collections of openings in different parts of the disk of the sun, and many ridges and nodules. The small indentations, as I have formerly called them, are so coarse, and of such irregular shapes, that they can be called so no longer. Corrugations, therefore, are that variety and unevenness of the whole surface of the sun, when it appears richly furnished with luminous clouds.

I am now much inclined to believe, that openings with great shallows, ridges, nodules, and corrugations, instead of small indentations, may lead us to expect a copious emission of heat, and therefore mild seasons. And that, on the contrary, pores, small indentations, and a poor appearance of the luminous clouds, the absence of ridges and nodules, and of large openings and shallows, will denote a spare emission of heat, and may induce us to expect severe seasons. A constant observation of the sun with this view, and a proper information respecting the general mildness or severity of the seasons, in all parts of the world, may bring this theory to perfection, or refute it, if it be not well founded.

Jan. 24, 1801. The surface of the sun is every where richly decked out with luminous clouds. An additional opening is lately come into view, attended by many spreading ridges.

Jan. 29, 1801. If openings be a sign of richness in the illuminating and heating disposition of the sun, there are enough of them: considerable ones are scattered in six different regions, taking up a broad zone.

Feb. 4, 1801. Between flying clouds, I counted 31 openings in the sun.

March 2, 1801. There are six different sets of openings in the sun. One of them consists of ten; another of two; the rest are single.

Coarse and luminous Corrugations.

Jan. 4, 1801. The corrugations are very coarse; and the luminous clouds seem to be very rich.

Jan. 3, 1801. The elevations of the corrugations are all very luminous, like so many nodules.

Feb. 18, 1801. The corrugations are every where very luminous.

March 2. The general surface of the sun is so rich, that the indentations are a good deal covered by self-luminous clouds.

From these two last sets of observations, one of which establishes the scarcity of the luminous clouds, while the other shews their great abundance, I think we may reasonably conclude, that there must be a manifest difference in the emission of light and heat from the sun. It appears to me, if I may be permitted the metaphor, that our sun has for some time past been labouring under an indisposition, from which it is now in a fair way of recovering. An application of the foregoing method, however, even if we were perfectly assured of its being well founded, will still remain attended with considerable difficulties.

We see how, in that simple instrument the barometer, our expectations of rain or fair weather, are only to be had by a consideration of many circumstances, besides its actual elevation at the moment of inspection.

The tides also present us with the most complicated varieties

in their greatest elevation, as well as in the time when they happen on the coasts of different parts of this globe. The simplicity of their cause, the solar and lunar attractions, we might have expected, would have precluded every extraordinary and seemingly discordant result.

In a much higher degree, may the influence of more or less light and heat from the sun, be liable to produce a great variety in the severity or mildness of the seasons of different climates, and under different local circumstances; yet, when many things which are already known to affect the temperature of different countries, and others which future attention may still discover, come to be properly combined with the results we propose to draw from solar observations, we may possibly find this subject less intricate than we might apprehend on a first view of it.

If, for instance, we should have a warm summer in this country, when phenomena observed in the sun indicate the expectation of it, I should by no means consider it as an unsurmountable objection, if it were shewn that in another country the weather had not been so favourable. And, if it were generally found that our prognostication from solar observations held good in any one given place, I should be ready to say that, with proper modifications, they would equally succeed in every other situation.

Before we can generalize the influence of a certain cause, we ought to confine our experiment to one permanent situation, where local circumstances may be supposed to act nearly alike at all times, which will remove a number of difficulties.

To recur to our instance of the tides, if we were to examine the phenomena which they offer to our inspection in any one given place, such as the mouth of the Thames, we should soon be convinced of their agreement with the motion of the sun and

moon. A little reflection would easily reconcile us to every deviation from regularity, by taking into account the direction and violence of winds, the situation of the coast, and other circumstances. Nor should we doubt the truth of the theory of the tides, though high water at Bristol, Liverpool, or Hull, should have been very deficient, at a time when, in the place of our experiments, it had happened to be uncommonly abundant.

Now, with regard to the effects of the influence of the sun, we know already, that in the same latitudes the seasons differ widely in temperature; that it is not hottest at noon, or coldest at midnight; that the shortest day is neither attended with the severest frosts, nor the longest day with the most oppressing heats; that large forests, lakes, morasses, and swamps, affect the temperature one way; and rocky, sandy, gravelly, and barren situations, in a contrary manner; that the seasons of islands are considerably different from those of large continents, and so forth.

But it will now be necessary to examine the accounts we already have of the appearance and disappearance of the solar spots, and to compare them with the temperature of the respective times, as far as history will furnish us with records.

The first thing which appears from astronomical observations is, that the periods of the disappearance of spots on the sun are of much shorter duration than those of their appearance; so that, if the symptoms which have been pointed out, as denoting the state of the sun with regard to light and heat, should be well founded, we ought rather to look upon the absence of spots as a sign of deficiency, than on their presence as one of abundance; and this would justify my expression, of the recovery of the sun from an indisposition, as being a return to its usual splendour.

In going back to early observations, we cannot expect to meet with a record of such minute phenomena as we have attended to. The method of viewing spots on the sun, by throwing their picture, in a dark room, on a sheet of white paper, is not capable of delicacy; nor were the direct views of former astronomers so distinct as, in the present improved state of the telescope, we can have them; a very imperfect account of solar spots may therefore be expected, considering our present inquiry, which would require complete observations of every spot, great or small, that has been on the sun during such periods as will be examined.

With regard to the contemporary severity and mildness of the seasons, it will hardly be necessary to remark, that nothing decisive can be obtained. But, if we are deficient here, an indirect source of information is opened to us, by applying to the influence of the sun-beams on the vegetation of wheat in this country. I do not mean to say, that this is a real criterion of the quantity of light and heat emanated from the sun; much less will the price of this article completely represent the scarcity or abundance of the absolute produce of the country. For the price of commodities will certainly be regulated by the demand for them; and this we know is liable to be affected by many fortuitous circumstances. However, although an argument drawn from a well ascertained price of wheat, may not apply directly to our present purpose, yet, admitting the sun to be the ultimate fountain of fertility, this subject may deserve a short investigation, especially as, for want of proper thermometrical observations, no other method is left for our choice.

Our historical account of the disappearance of the spots in the

sun, contains five very irregular and very unequal periods.* The first takes in a series of 21 years, from 1650 to 1670, both included. But it is so imperfectly recorded, that it is hardly safe to draw any conclusions from it; for we have only a few observations of one or two spots that were seen in all that time, and those were only observed for a short continuance. However, on examining the table of the prices of the quarter of nine bushels of the best or highest priced wheat at Windsor, marked in Dr. ADAM SMITH's valuable Inquiry into the nature and causes of the wealth of nations,† we find that wheat, during the time of the 21 years above mentioned, bore a very high price; the average of the quarter being £2. 10*s*. $5\frac{12}{21}$*d*. This period is much too long to suppose that we might safely compare it with a preceding or following one of equal duration. Besides, no particulars having been given of the time preceding, except that spots in the sun, a good while before, began to grow very scarce, there might even be fewer of them than from the year 1650 to 1670. Of the 21 years immediately following, we know that they certainly comprehend two short periods, in which there were no spots on the sun; of these, more will be said hereafter; but, including even them, we have the average price of wheat, from 1671 to 1691, only £ 2. 4*s*. $4\frac{2}{3}$*d*. the quarter. The difference, which is a little more than as 9 to 8, is therefore still a proof of a temporary scarcity.

Our next period is much better ascertained. It begins in December 1676, which year therefore we should not take in, and goes to April 1684; in all which time, Flamsteed, who was then observing, saw no spot in the sun. The average price of wheat, during these 8 years, was £ 2. 7*s*. 7*d*. the quarter. We

* See Astronomie par M. De la Lande, § 3235. † See Book I. Chap. XI.

cannot justly compare this price with that of the preceding 8 years, as some of the former years of scarcity would come into that period; but the 8 years immediately following, that is, from 1685 to 1691, both included, give an average price of no more than £1. 17*s.* 1$\frac{3}{4}$*d.* The difference, which is as full 5 to 4, is well deserving our notice.

A third but very short period, is from the year 1686 to 1688, in which time Cassini could find no spot in the sun. If both years be included, we have the average price of wheat, for those three years, £1. 15*s.* 0$\frac{2}{3}$*d.* the quarter. We ought not to compare this price with that of the three preceding years, as two of them belong to the preceding period of scarcity; but the three following years give the average price for the quarter of wheat £1. 12*s.* 10$\frac{2}{3}$*d.* or, as nearly 11 to 10.

The fourth period on record, is from the year 1695 to 1700, in which time no spot could be found in the sun. This makes a period of 5 years; for, in 1700 the spots were seen again. The average price of wheat, in these years, was £3. 3*s.* 3$\frac{1}{5}$*d.* the quarter. The 5 preceding years, from 1690 to 1694, give £2. 9*s.* 4$\frac{4}{5}$*d.* and the 5 following years, from 1700 to 1704, give £1. 17*s.* 11$\frac{1}{5}$*d.* These differences are both very considerable; the last is not less than 5 to 3.

The fifth period extends from 1710 to 1713; but here there was one spot seen in 1710, none in 1711 and 1712, and again one spot only in 1713. The account of the average price of wheat, for these four years, is £2. 17*s.* 4*d.* the quarter. The preceding four years, from 1706 to 1709, give the price £2. 3*s.* 7$\frac{1}{2}$*d.* and the following years, from 1714 to 1717, it was £2. 6*s.* 9*d.* When the astronomical account of the sun for this period, which has been stated above, is considered, these two differences will

be found very considerable; the first of them being nearly as 4 to 3.

The result of this review of the foregoing five periods is, that, from the price of wheat, it seems probable that some temporary scarcity or defect of vegetation has generally taken place, when the sun has been without those appearances which we surmise to be symptoms of a copious emission of light and heat. In order, however, to make this an argument in favour of our hypothesis, even if the reality of a defective vegetation of grain were sufficiently established by its enhanced price, it would still be necessary to shew that a deficiency of the solar beams had been the occasion of it. Now, those who are acquainted with agriculture may remark, that wheat is well known to grow in climates much colder than ours; and that a proper distribution of rain and dry weather, with many other circumstances which it will not be necessary to mention, are probably of much greater consequence than the absolute quantity of light and heat derived from the sun. To this I shall only suggest, by way of answer, that those very circumstances of proper alternations of rain, dry weather, winds, or whatever else may contribute to favour vegetation in this climate, may possibly depend on a certain quantity of sun-beams, transmitted to us at proper times; but, this being a point which can only be ascertained by future observations, I forbear entering farther into a discussion of it.

It will be thought remarkable, that no later periods of the disappearance of the solar spots can be found. The reason however is obvious. The perfection of instruments, and the increased number of observers, have produced an account of solar spots, which, from their smallness, or their short appearance, would probably have been overlooked in former times. If we should

in future only reckon the years of the total absence of solar spots, even that remarkable period of scarcity which has fallen under my own observation, in which nevertheless I have now and then seen a few spots of short duration, and of no great magnitude, could not be admitted.

For this reason, we ought now to distinguish our solar observations, by reducing them to short periods of symptoms for or against a copious emission of the solar beams, in which, all the phenomena we have pointed out should be noticed. The most striking of them are certainly the number, magnitude, and duration of the openings. The increase and decrease of the luminous appearance of the corrugations is perhaps full as essential; but, as it is probable that their brilliancy may be a consequence of the abundance of the former phenomena, an attention to the latter, which is subject to great difficulties, and requires the very best of telescopes, may not be so necessary.

What remains to be added is but short. In the first of my two series of observations, I have pointed out a deficiency in what appears to be the symptomatic disposition of the sun for emitting light and heat: it has lasted from the year 1795 to 1800.* That we have had a considerable deficiency in the vegetation of grain, will hardly require any proof. The second series, or rather the commencement of it, for I hope it will last long, has pointed out a favourable return of the rich appearance of the sun. This, if I may venture to judge, will probably occasion a return of such seasons as, in the end, will be attended by all their usual fertility.

The subject, however, being so new, it will be proper to

* This period should properly have been divided into two small ones; but, for want of intermediate solar observations, I have joined the visible deficiencies in the illuminating and heating powers of the sun, from the year 1795 to 1796, and again from 1798 to 1800, into one.

conclude, by adding, that this prediction ought not to be relied on by any one, with more confidence than the arguments which have been brought forwards in this Paper may appear to deserve.

EXPLANATION OF THE 1st, 2d, 11th and 12th FIGURES.

SEE PLATES XVIII. and XIX.

Plate XVIII. Fig. 1, represents an opening in the luminous solar clouds, with its surrounding shallow, in a situation much past the centre, towards the preceding limb of the sun. The lines marked with the letters *a*, *b*, *c*, *d*, *e*, *f*, answer to those which are marked with the same letters in Fig. 2.

Figure 2, is a section of the same opening. The lines *a*, *b*, *c*, *d*, *e*, *f*, are supposed to be drawn from the eye of the observer. *a* and *b* point out the elevation of the corrugations *g*, *h*, on the preceding side P, above the surface of the shallow *i*, *k*. *c* and *d* shew the thickness of the shallow; and the line *d* goes through the opening, down into the clear atmospheric space P F, till it meets with the opaque surface of the sun A B. On the following side F, the thickness of the shallow and elevation of the corrugations cannot be seen; since the line *e* goes abruptly into the opening; and *f* goes, as abruptly, from the top of the corrugations, down to the shallow.

Plate XIX. Fig. 11, shews a section of the corrugations, shallow, and opening, of Fig. 10, in the same manner as Fig. 2 represents those of Fig. 1. There is a hanging cloud *a*, in Fig. 11, over the preceding part of the opening; and the same cloud is represented at *b*, in Fig. 12, to which place it was seen to move from its former situation, in 58 minutes of time.

The rest of the figures are sufficiently explained in the places of the text which refer to them.

Fig. 2.

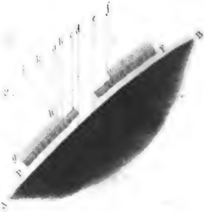

Fig. 4.

Fig. 6.

Fig. 7.

Fig. 9.

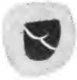

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Fig. 11.

Fig. 13.

Fig. 15.

Fig. 16.

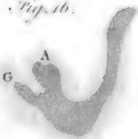


Fig. 18.

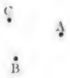


Fig. 19.

Basire sc.

XIV. *Observations on the Structure, and Mode of Growth, of the grinding Teeth of the Wild Boar, and Animal incognitum.* *By* Everard Home, *Esq. F.R.S.*

Read May 7, 1801.

THE peculiarities in the mode of growth of the grinding teeth of the Sus Ethiopicus have, upon a former occasion, been laid before this learned Society, and the similarity of their structure to those of the elephant explained.

The wild boar has been since discovered to have similar peculiarities, although in a less degree, and taking place at a later period of the animal's life: an account of these is the subject of the present Paper.

When the peculiarities just alluded to were noticed in the teeth of the Sus Ethiopicus, it naturally led to the examination of the grinding teeth in the different species of Sus; and, that no such peculiarities should be found in any of them, appeared undoubtedly very extraordinary; but, upon reflecting that they only belonged to the second set of teeth, and that the first or temporary teeth of the Sus Ethiopicus were exactly similar to those of the common hog, the idea suggested itself, that a similar change might take place in the teeth of the other species, at a more advanced period of life; and, that it had not been detected in the heads that had been examined, because the animals were too young.

This view of the subject encouraged me to prosecute the enquiry, and made me desirous of examining the grinding teeth of the wild boar in the different stages of their growth, and, if possible, after the animal had arrived at an advanced age.

My wishes respecting the wild boar were mentioned to Sir JOSEPH BANKS, who very obligingly sent me two sculls he had received from Germany; and GEORGE BEST, Esq. F. R. S. was so good as to send over to Hanover, for the head of one of the largest boars that could be procured: his request was immediately complied with, and the animal to which the head belonged, was considered by the hunters as under seven years of age.

From an examination of these different specimens, I have been able to make out very satisfactorily, the mode of dentition of the wild boar during the first seven years; and to ascertain, that there is a succession of grinding teeth beyond that period.

In this species of Sus, the temporary grinders consist of sixteen; four on each side, both of the upper and under jaw.

These sixteen teeth are shed in the usual manner, and their places supplied by larger teeth rising up from the substance of the jaw, immediately under the old ones.

Before these first teeth are shed, one of the more permanent grinders is formed, in the posterior part of each side of both the upper and under jaw; this tooth, although it is in its place with the first set, is to be considered as belonging to the second set.

In explaining the subsequent changes which take place, I shall confine myself to the lower jaw; as the figures which are annexed are taken from the teeth in that jaw.

Of the five teeth on each side of the lower jaw, one is separated from the rest, and is close to the tusk, which admits of a space, for the curve of the upper tusk to rest upon; so that

there are, properly speaking, only four grinders, forming a regular row.

As the jaw increases in length, a small cell is formed in its substance, behind the last grinder, in which the rudiments of a new tooth appear: these increase, along with the cavity in which they are contained; and the new tooth is in every respect larger than the preceding one. By the time it is completely formed, and ready to cut the gum, the jaw has extended itself, so that there is room for it to come into its place, as the posterior grinder.

While this tooth is concealed in the jaw, another cell is formed immediately beyond it; and there is a small round hole of communication between the two cells, similar to what is met with in the elephant; but there are no remains of such a communication, between the anterior cell and the socket of the full grown tooth immediately before it.

The last mentioned cell is at first very small; but gradually increases to a prodigious size; and the tooth formed in it is nearly double the size of the preceding large grinder. Its masticating surface has a row of four projections on each side, and the tooth has eight fangs; so that it very much resembles two large grinding teeth incorporated into one: the posterior fangs are not completely formed at seven years of age.

This large tooth, although it is formed in the posterior part of the jaw, is brought sufficiently forward, by the growth of the jaw bone, to cut the gum, and range in the line with the other teeth, making the connected row of grinders six in number. From its very great size, it not only fills the jaw completely, but all the bodies of the other five teeth are pushed by it out of their perpendicular direction, leaning a little forwards.

As soon as the sixth grinder has cut the gum, a new cell begins to appear immediately beyond it, to receive the rudiments of another tooth.

This last cell, at seven years of age, is very small; and the specimens in my possession do not enable me to prosecute the enquiry; but there is every reason to believe the tooth formed in it, equals or exceeds the large one that has been described.

The appearances which have been mentioned will be better understood by referring to the annexed figures, (Plates XX, XXI, XXII, and XXIII.) than by any verbal description.

The large grinder in the wild boar resembles those of the Sus Ethiopicus and elephant, in having a larger extent of masticating surface than is met with in other teeth; it differs, however, materially in its structure, having that surface composed of a strong crust of enamel, with projecting transverse ridges; showing that it is not intended for grinding simply, but belongs to an animal which, like the human species, is fitted to live on both animal and vegetable substances.

Previous to a tooth of this structure having been discovered with an extended masticating surface, the curious mechanism of the elephant's grinders appeared to be the only one peculiarly fitted for that purpose; but we now find, that the more usual structure of grinding teeth will admit their surfaces to be extended, whenever their enlargement becomes necessary to enable the animal to prepare the food for the process of digestion.

The elephant, the Sus Ethiopicus, and the wild boar, are the only recent animals in which this extended masticating surface of the grinding teeth has been met with. There is nothing

similar to it in the bear; and, in the lion and tyger, the posterior grinder has a small tooth placed close to it, which is a different mode of increasing the grinding surface.

The rhinoceros and hippopotamus have no increase of the surface of the posterior grinders, beyond what is usually met with.

The tapir I have not had an opportunity of examining in its full grown state; so that I am unable to say what extent the masticating surfaces of the posterior grinders may acquire.

In the human species, the mode of dentition is upon the same principle as in the wild boar; only the last-formed grinding teeth in each jaw, called dentes sapientiæ, from the late period of life at which they cut the gum, do not in size exceed the others, but are rather smaller, and very often have not sufficient room in the jaw to come into their regular place, although they do not make their appearance till between twenty and thirty years of age.

In the negro, the dentes sapientiæ have sufficient room to come into their place, and are in general full as large as the other grinders; the growth of the posterior part of the jaw being evidently greater than in the European.

When the age of man was much greater than at present, it is natural to suppose the growth of the posterior part of the jaw was continued for a longer time, and the space for the dentes sapientiæ was more extensive. Under such circumstances, these teeth would probably be large, in proportion to the space which was to receive them; and when, instead of threescore and ten, a thousand years was the period of a man's life, we should be

led to conclude, from the preceding observations, that there was a succession of dentes sapientiæ.

There is a very curious circumstance in favour of this conjecture, which has been mentioned to me by Sir JOSEPH BANKS. In Otaheite, the natives have a tradition, that Adam, or the first man, was remarkable for the length of his jaws. His name, in their language, is *Taa roa tabi etoomoo,* which signifies the one (the stock) from which all others sprung, with the long jaws; so that these islanders have a tradition of the original race of men having had their jaws much longer than at present.

Although the grinder of the boar differs in appearance, as in extent of surface, from those of all the recent animals that have been mentioned, yet, upon comparing it with the large fossil teeth found on the banks of the river Ohio in North America, belonging to the animal incognitum, they are so much alike, both in their external appearance and internal structure, that it is evident they are teeth of the same kind, only of very different sizes.

This resemblance led me to examine the mode of dentition of this unknown animal, as far as could be done from the specimens preserved in this country, to see if any resemblance could be traced between it and that of the wild boar.

From the different specimens of these fossil teeth deposited in the British Museum, the collection of the late Dr. WILLIAM HUNTER, and the HUNTERIAN Museum, together with one in my own possession, presented to me by Sir JOSEPH BANKS, the following facts have been ascertained.

The first grinders are small, when compared with those which

are afterwards formed, being scarcely more than half their size: they have three transverse projecting ridges, completely encrusted with enamel, as well as every other part of the masticating surface.

Two of these grinders, and probably more, are present on each side of the jaw at the same time. As the animal increases in size, and the jaw extends itself, a larger kind of grinder is formed in the posterior part of the jaw, exactly similar to what happens in the elephant; and, as this large tooth, which has five projecting transverse ridges on the masticating surface, becomes completely formed, it comes forward, and occupies the principal seat in the jaw, and the others drop out.

When the smaller grinders are examined, the greater number of them have their fangs all bent in one direction, in consequence of the bodies of the teeth having been pushed forward, by the large posterior tooth coming into their place, as was observed in the temporary grinders of the young Sus Ethiopicus.

This process is well illustrated by two specimens, which show the teeth in the two stages of growth, and which are represented in the annexed figures.

The first is a lower jaw preserved in the HUNTERIAN Museum, in which the two small grinders are in their sockets; and the cavity for the formation of the large grinder has, upon its sides, the impression of the different parts of the body of the tooth.

The second is a lower jaw in the British Museum, in which the large grinder is completely formed, and occupies the principal part of the jaw, at the anterior part of which are the

remains of the sockets, from which the smaller grinders had fallen out.

This mode of dentition is precisely similar to that of the elephant; and, in the structure of the tooth, it resembles that of the boar; we have therefore complete evidence of a tooth of this last structure acquiring the size of that of the elephant, and succeeding those which preceded it in the same manner.

The animal incognitum, with respect to its teeth and the mode of their succession, being an intermediate step between the elephant and wild boar, both of which have tusks, gives a degree of probability to the opinion which has been very generally adopted, of this animal also having tusks.

Of this, however, there is at present no confirmation; nor is there sufficient ground for denying their existence; the part of the upper jaw in which they ought to be situated not having been preserved entire, in any of the specimens that have come under my observation.*

That tusks have been found, resembling those of the elephant, in the same places in which the fossil teeth were met with, proves nothing; since fossil elephants' grinders have been found in the same situation.

The scull of the fossil skeleton found in South America, a description of which is given by Mr. Cuvier, Secretary to the National Institute at Paris, in size resembles that of the animal incognitum; but, when it is particularly examined, it will be found that the animal is of a very different genus. The shape

* From the appearance of the lower jaw in the British Museum, there is sufficient evidence to ascertain that there is no tusk in the lower jaw.

of the lower jaw is totally unlike; and there are four grinding teeth on each side of the jaw, with flat crowns, on which are transverse grooves; which shows that the number and appearance of the teeth are very different from those of the animal incognitum of North America. This South American animal incognitum, therefore, having no tusks, cannot be brought in evidence either for or against their existence in the other.

The mode of dentition by a succession of large grinders formed in the posterior part of the jaw, is not the only one adopted by nature for the supply of those animals who live to a great age, and require a renewal of their teeth, with an increase of size proportioned to the enlarged growth of the animal.

Crocodiles and seals live to a considerable age, and grow to a very large size; but, from the nature of their food, their teeth are small; and therefore, in those animals, the succession is from the portion of the jaw immediately under those which are to be shed. As the jaw becomes larger, the teeth increase in size, but never so much as to prevent there being room for the growth of the new tooth under the old, although the succeeding teeth are three or four times larger than the original ones, in both these genera of animals.

The mode of dentition in the elephant, animal incognitum, and boar, appears to be confined to those animals of great longevity, whose food has so much resistance as to require the teeth being of a size too large to admit of the new tooth and the old being contained in the same portion of the jaw, at the same time.

That the elephant lives to a great age, is sufficiently ascertained; and the size of the bones of the animal incognitum, is almost sufficient evidence of its being a long lived animal.

The wild boar of Germany, from living in a savage state, cannot have its natural life appreciated with any accuracy; but, if we may credit the accounts recorded, of the size to which it grows, it may be presumed that many years are necessary for that purpose.

The following statements upon this subject, have been communicated to me by Mr. BEST, from Hanover.

In the year 1581, a boar was killed near Koningsberg, in Prussia, of six hundred pounds weight.

In 1507, one was killed in the dukedom of Wirtemberg, seven feet three inches long, by five feet three inches high. The length of the head was twenty-three inches.

From these accounts of the enormous size of the wild boar in the 16th century, it cannot be doubted that the animal, where its haunts are not disturbed by hunters, lives to a great age; if that were not the case, the mode by which its teeth are renewed, would be entirely unnecessary.

A boar of this description, matured in its native forests, when it arrived at the age of 60 or 100 years, possessed of the strength and sagacity to be acquired in that time, must have been an animal more formidable than any which are at present to be met with; and, when it made occasional excursions into the nearest cultivated lands, it must have excited the greatest degree of terror and alarm among the inhabitants.

Before the use of fire-arms, it is not at all improbable that such an animal should drive before it the peasantry of a whole district; and that the boldest warriors should be solicited to come from the neighbouring cities, to put a stop to its ravages.

The histories of this kind which are to be met with in the works of the ancient poets and historians, are therefore not

to be considered as wholly fabulous, but the recital of events which really happened, although probably in many instances much exaggerated and embellished.

Ovid's description of the wild boar killed by Meleager, which he asserts to be larger than the Sicilian bulls, with tusks equal in size to those of the elephant, was probably taken from some Greek account, which, being founded on tradition, may be supposed, from the preceding observations, to have been in its origin a true history.*

It is deserving of remark, in proof of the Roman poets having considered the wild boar as an animal that lived to a great age, and grew to an enormous size, that, while Ovid gives the particulars of its bulk, Virgil thinks it sufficient, when he means to describe the animal in all its power, to say it had lived many years, without at all particularizing its size.†

* Inquit; et Œneos ultorem spreta per agros
Misit aprum, quanto majores herbida tauros
Non habet Epiros; sed habent Sicula arva minores.
Sanguine et igne micant oculi, riget horrida cervix,
Et setæ densis similes hastilibus horrent;
Stantque velut vallum, velut alta hastilia setæ.
Fervida cum rauco latos stridore per armos
Spuma fluit; dentes æquantur dentibus Indis.

Ovid. Metam. lib. viii. l. 281

† Ac velut ille canum morsu de montibus altis
Actus aper, (multos Vesulus quem pinifer annos
Defendit, multosque Palus Laurentia,) sylvâ
Pastus arundineâ; postquam inter retia ventum est,
Substitit, infremuitque ferox, et inhorruit armos.
Nec cuiquam irasci, propiusve accedere virtus;
Sed jaculis, tutisque procul clamoribus instant.

Virg. Æn. lib. x. l. 707.

The observations which have been made upon the wild boar, are laid before this learned Society, with a view of calling the attention of other physiologists to this subject, whose opportunities may enable them to prosecute it with more advantage.

From this consideration, these materials, so inadequate to the extent of the enquiry, have been brought forward; as many years might elapse before more could be procured; and the hints that have been thrown out may induce others to assist in collecting facts upon this subject.

EXPLANATION OF THE PLATES.

Plate XX. In this Plate are three figures of the lower jaw of the wild boar, in different stages of growth, to shew the mode in which the teeth are supplied. The figures are on a scale of half an inch to an inch.

Fig. 1. Represents the first set of grinders, and the mode in which they are shed, by others rising up immediately under them: one of the second set is in its place; another is forming in the substance of the jaw; and there is a small cell behind it, in which were the rudiments of the succeeding tooth.

Fig. 2. Represents the second set of teeth in their place, with the small cell delineated in the first figure increased to a large size, and containing an imperfectly formed tooth, greatly exceeding in size any of the others.

Fig. 3. Represents the jaw in a still more advanced stage of its growth, with the tooth which was only forming in the second figure now come to its full size, and in its proper place in the row of teeth; there is also a new cell formed, for a succeeding tooth.

Plate XXI. Represents a view of a portion of the lower jaw of the animal incognitum, (on a scale of half an inch to an inch,) to shew the appearance of the smaller or first formed grinders, in their place in the jaw; also the cavity immediately behind them in the substance of the jaw, in which were contained the rudiments of the larger grinder, which was afterwards to occupy the principal part of the jaw.

Plate XXII. Represents a view of the lower jaw of the animal incognitum, with the large grinder, (the cavity for forming

which was seen in the last Plate,) now come to perfection, and occupying its proper place in the jaw; there are also imperfect remains of the sockets of the smaller grinders. In this Plate the figure is on a scale of one-third of an inch to an inch.

Plate XXIII. Represents a section of the grinding tooth of the animal incognitum, (on a scale of half an inch to an inch,) to shew the internal structure, and the mode in which the tooth is formed upon the pulp. From this section it is evident, that this tooth, like that of the elephant, does not arrive at perfection in all its parts at the same time: one portion of the tooth is first completed, and afterwards, in succession, the other parts. The part which is nearly solid is the first formed; and the large cavity contained the pulp upon which the rest of the body of the tooth was to have been made solid.

Fig. 1

Fig. 2

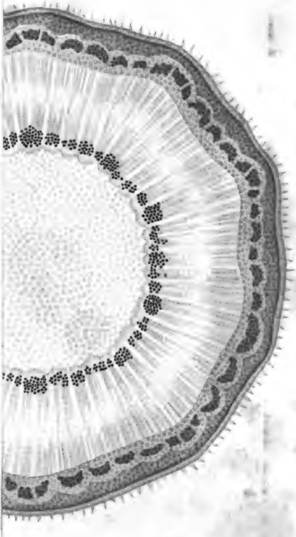
Fig. 3

J. Basire sc.

F. Basire sc.

J. Basire

J. Basire sc.



Basire sc.

XV. *Account of some Experiments on the Ascent of the Sap in Trees. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read May 14, 1801.

MY DEAR SIR,

THE cause of the ascent of the sap in trees appearing to me not to have been satisfactorily accounted for, I have lately turned my attention to that subject; and, as some facts have come under my observation, which do not appear to have been noticed by any author that I have seen, I take the liberty to trouble you with an account of a few of the experiments that I have made; hoping that some of them may appear new and interesting to you. These experiments were made on different kinds of trees; but I shall confine myself to those made on the crab-tree, the horse-chesnut, the vine, and the oak; and shall begin with those made on the crab-tree.

Choosing several young trees of this species in my nursery, of something more than half an inch diameter, and of equal vigour, I made two circular incisions through the bark, round one half the number of them, about half an inch distant from each other, early in the spring of 1799; and I totally removed the bark between these incisions, scraping off the external coat of the wood. The other half I left in their natural state.

At the usual season, the sap rose in equal abundance in all;

and their branches shot, during the whole spring, with equal luxuriance. But that part of the stems (of the trees whose bark had been taken off) which was below my incisions, scarcely grew at all; whilst all the parts above the incisions, increased as rapidly as in the trees whose bark remained in the natural state: the upper lips of the wounds also made considerable advances towards an union; but the lower ones made scarcely any.

Soon after Midsummer, those parts of the wood which had been deprived of bark became dry and lifeless, to some depth; and the sap, in consequence, meeting obstruction in its ascent, some latent buds shot forth, in some of the plants, below the incisions When one of the shoots which these buds produced was suffered to remain, the part of the stem below it began immediately to increase in size; but, if it was at any distance below the incision above, the part between it and that incision still remained very nearly stationary, so as to be, in the autumn, almost a whole year's growth less than the stem above the incisions.

Choosing other stocks, which had each a strong lateral branch, I removed the bark, in the manner described, in two places; the one above, and the other below, each lateral branch. The sap here passed both my incisions, as freely as in the former experiment; the lateral branches between them grew with the greatest vigour; and the part of the stem between those branches and the lower incisions increased much in size. I varied these experiments in every way that occurred to me; and the result uniformly was, that those parts of the stems and branches which were above the incisions, and had a communication with the leaves, through the bark, increased rapidly; whilst those

below the incisions scarcely grew at all, till a new communication with the leaves through the bark was obtained, by means of a lateral shoot, below the incisions. It now appeared to me to be probable, that the current of sap which adds the annual layer of wood to the stem, must descend through the bark, from the young branches and leaves; and to these my attention was in consequence directed.

Towards the end of the summer, when some young luxuriant shoots of my apple-trees had attained a proper degree of firmness, I made four circular incisions through the bark of each, as in the preceding instances; and I removed the bark in two places, leaving a leaf between the places where the bark was taken off. Examining them frequently during the autumn, I found that the insulated leaf acted just as the lateral branch had done; the part of the bark and stem between it and the lower incision being apparently as well fed as any other part of the tree; and it grew as much. Making similar incisions on other branches of the same age, I left similar portions of insulated bark, without a leaf between the incisions; but in those no apparent increase in the size of the wood was discoverable.

I was still unacquainted with the channel through which the sap was conveyed into the leaf; and therefore, having obtained a deeply tinged infusion, by macerating the skins of a very black grape in water, I prepared some annual shoots of the apple, and of the horse-chesnut, in the manner above mentioned; then, cutting them off a few inches below the incisions of the bark, I placed them for some hours in the coloured infusion. Making transverse sections of them afterwards, I found that the infusion had passed up the pores of the wood, beyond both my incisions, and into the insulated leaves; but it had neither coloured the bark, nor the sap between it and the wood; and

the medulla was not affected, or at most was very slightly tinged at its edges.

My attention was now turned to the leaves: these, in the apple-tree, are attached to the wood by three strong fibres or tubes, (or rather bundles of tubes,) one of which enters the middle of the leaf-stalk, and the others are on each side of it. In the horse-chesnut, there are seven or eight bundles of a similar kind of tubes in each leaf: through these the infusion had passed, and had communicated its colour to them, through almost the whole length of each leaf-stalk. Examining these tubes more minutely, I found that they were surrounded with others, which were free from colour, and appeared to be conveying, in one direction or the other, a different fluid. On tracing these downwards, I discovered that they entered the inner bark, and had no immediate communication with the tubes of the wood. I now endeavoured, in the same manner, to trace back those vessels which had carried the infusions into the leaves; and I readily found them to be perfectly distinct from the common tubes of the alburnum. They commence a few inches below the leaf to which they belong; and they become more numerous as they approach it; every where surrounding the medulla in bundles, as represented in Plate XXIV. To these vessels, the spiral tubes are every where appendages. I do not know that any specific name has been given to these vessels; and therefore, as they constitute a centre, round which the future alburnum is formed in the succulent annual shoot, I will call them the central vessels, to distinguish them from the spiral tubes and the common tubes of the wood. In Plates XXV. and XXVI. the direction of these vessels, with the spiral tubes, in their passage from the sides of the medulla to the leaf-stalk, is delineated in a transverse and longitudinal section: they extend to the extremities of the

leaf, where I believe they terminate. Plate XXVII. presents two sections of the leaf-stalk of the horse-chesnut; the first being taken from the middle of the stalk; and the second from its base. Lying parallel with, and surrounding the abovementioned vessels, appear other vessels, which I conclude return the sap to the tree; for, when a leaf was cut off which had imbibed a coloured infusion, I found that the native juices of the plant flowed from these vessels, apparently unaltered, as has been remarked by Dr. Darwin. These vessels descend through the inner bark, (as delineated in Plates XXIV. XXV. and XXVI.) and appear to extend from the extremities of the leaves to the points of the roots.

The whole of the fluid, which passes from the wood to the leaf, seems to me evidently to be conveyed through a single kind of vessel; for the spiral tubes will neither carry coloured infusions, nor in the smallest degree retard the withering of the leaf, when the central vessels are divided. But the annexed figures appear to point out at least two kinds of returning vessels. And I think it by no means improbable that two kinds exist, with distinct offices; for there is a new layer of alburnum, and a new internal bark, to be formed. I have, however, seen it asserted somewhere, in the writings of Linnæus, and other naturalists, that the internal bark is annually converted into alburnum. But this is totally erroneous; and a vigorous shoot of the apple-tree often presents in its transverse sections, when three or four years old, as many layers in its bark, each of which once formed its internal vascular lining.

As the bark appeared to me now to receive its nutrition through the leaf, I wished to see what effect would be produced by gradually reducing the quantity of the leaves. I had a luxu-

riant shoot of the vine in my vinery, exactly in the stage of growth I wanted; and this branch therefore was, towards its point, every day deprived of a small portion of its leaf. The bark, in consequence, became shrivelled and dry; and at length the buds below vegetated; and the point of the shoot died, apparently for want of nourishment. I here observed, as I had frequently done before, that almost the whole action of each leaf lies between itself and the root; for the branch, in this case, was perfectly well fed below the uppermost unmutilated leaf, but failed immediately above it.

Every branch in which I had yet attempted to trace the progress of the sap having contained its medulla uninjured, the action of that substance next engaged my attention, and I made the following experiments on the vine. Having made a passage about half an inch long, and a line wide, into a strong succulent shoot of this plant, I totally extracted its medulla, as far as the orifice I had made would permit me. But the shoot grew nearly as well as the others, whose medulla had remained uninjured; and the wound soon healed. Making a similar passage, but of greater length, so that part extended above, and part below, a leaf and bud, I again extracted the medulla. The leaf and bud, with the lateral shoot annexed, (in the vine,) continued to live, and did not appear to suffer much inconvenience; but faded a little when the sun shone strongly on them.

I was now thoroughly satisfied, that the medulla was not necessary to the progression of the sap; but I wished to see whether the wood and leaf could execute their office when deprived at once of the bark and medulla. With this view, I made two circular incisions through the bark, above and below a leaf; and I took off the whole of the bark between them,

except a small portion round the base of the leaf. Having then perforated the wood, where I made each of my incisions through the bark, I destroyed the medulla in each place, as in the preceding experiments. The leaf, however, continued fresh and vigorous; and a thin layer of new wood was formed round its base, as far as the bark had been suffered to remain.

Whilst I was waiting the result of the preceding experiments, I made a few efforts to discover another branch of circulation, namely, that which takes place within the fruit, and conveys nourishment to the future offspring. My experiments were here, however, confined almost entirely to two species of fruit, the apple and the pear; and, therefore, as the organization of different fruits is evidently different, I do not consider my observations such as can throw much general light on the subject. Examining the fruit-stalks of the apple, the pear, the vine, and some other fruit-trees, I found their organization to be nearly similar to that of the branch from which they sprang, and to consist of the medulla, the central tubes, a very small portion of wood, the spiral tubes and those of the bark, and the two external skins. Tracing the progress of these in the full-grown fruits of the apple and pear, I found, as LINNÆUS has described, that the medulla appeared to end in the pistilla. The central vessels diverged round the core, and, approaching each other again in the eye of the fruit, seemed to end in ten points at the base of the stamina, to which I believe they give existence. The spiral tubes, which are in all other parts appendages to these vessels, I could not trace beyond the commencement of the core; but, as the vessels themselves extend through the whole fruit, it is probable that the spiral tubes may have escaped my observation. LINNÆUS supposes the stamina to arise from the

wood. I should not venture to state an opinion in opposition to his; but I believe he has not any where distinguished those I call the central vessels, from the common tubes of the wood.

Having hitherto found that all advancing fluids appeared to pass either along the tubes of the alburnum, or along the central vessels, I had little doubt that the fruit was fed through the latter; but my efforts to ascertain this, in the autumn of 1799, were not successful. In the last spring, I was more fortunate. Placing small branches of the apple, the pear, and the vine, with blossoms not yet expanded, in a decoction of logwood, I found that the colouring matter readily passed up the central tubes of the fruit-stalks of all; and, in the apple and pear, I easily traced it, through the future fruit, to the base of the stamina. The office of the tubes in the bark did not appear in this experiment; but, as I have reason to believe the motion of the sap in the bark to be always retrograde, I am disposed to conclude that it is so here, and that, through the bark of the stalk, any superfluous humours existing in the fruit, from excessive humidity of weather, or other cause, is carried back, and absorbed by the tree. I have, however, very frequently repeated an experiment on the vine, which, I think, evidently proves that the fluid returned (if any) is essentially different from that which is derived from the leaf. In the culture of this fruit, I have frequently pinched off the young shoot, immediately above a bunch, as soon as the latter became visible in the spring, letting the leaf opposite the bunch remain. In this case, the wood below the upper leaf acquired nearly its proper length and substance. But, when I have taken off that leaf, the wood between the bunch and the next leaf below, has ceased to elongate; and has

remained, in form and substance, similar to the small fruit-stalk attached to it.

I was long at a loss to conjecture by what means nutrition was conveyed to the seeds of the apple and pear; for I had reason to believe that it was not done by the medulla; and I had previously ascertained that the seeds would derive nourishment from the pulp, when the fruit was taken prematurely from the tree. At length, in a large apple, which was just beginning to decay, I found a number of minute vessels, leading from the pulp to the tubes which originally constituted the lower parts of the pistilla, and to which the seeds are attached. These now appeared to me evidently to be the channels of nutrition to the seeds; and, since I have known what I have to look for, I find these vessels sufficiently visible in every apple: there are, however, five other tubes, which pass along the external edges of the cells of the core, to which I do not venture to assign an office. It appears to me not very improbable, that the internal organization of this fruit will be found to bear some resemblance to the placenta and umbilical cord of the animal economy. If transverse and longitudinal sections of young apples and pears be made, soon after the blossom has fallen, the pulp will appear to be of two kinds; one of which is included within the vessels which carry up coloured infusions; and this seems to be formed by continuation of the vessels and fibres within the wood. The other part appears to belong, in a great measure, to the bark: it is in very small quantity in the very young fruit; but, at its maturity, it constitutes much the greater part of the pulp. The vessels, however, which diverge into the external pulp, and probably convey nourishment to it, appear to be continuations of the central vessels, every where, I believe, accompanied, as in

the leaf, with minute ramifications of the tubes of the bark. The substance of the core is similar to that of the silver grain of the wood, of which it may possibly be a continuation.

The force with which the sap has been proved to ascend, by HALES, banishes every idea of mere capillary attraction. The action of the spiral tubes appears much more adequate to the effects produced; and I readily admit the supposed action of these, wherever they are found; but I have so often attentively searched in vain for them, with glasses of different powers, in the root, in the alburnum, and in the bark, that I cannot but question their existence in those parts. Attached to the central vessels, in the annual shoot, in the fruit-stalk of different trees, in the tendril of the vine, in the leaf, and in the seed, the spiral tubes certainly exist, and are in most cases visible, without the aid of a lens. But, as I have not been able to discover them in other parts of the tree, and as the different authors I have looked into have not distinguished those I call the central vessels from the common tubes of the alburnum, nor marked the difference in the organization of the annual branch, and annual root, I must venture to call their accuracy here in question, though with great deference for their opinions.

LINNÆUS and others have attempted to account for the ascent of the sap, by the expansion of the fluids within the vessels of the plant, by the agency of heat. But the sap rises under a decreasing, as well as under an increasing temperature, during the evening and night, (if it be not excessively cold) as well as in the morning and at noon; and it is sufficiently evident, that the heat applied to the branches of a vine within the stove, cannot expand the fluids in the stems and roots, which grow on the outside. It is also well known, that the degree of heat required

to put the sap into motion, in this plant, is not definite, but depends on that to which the plant has been previously accustomed. Thus, a vine which has grown all the summer in the heat of a stove, will not be made to vegetate during the winter by the heat of that stove: but, if another plant of the same variety, which has grown in the open air, be at any time introduced, after it has dropped its leaves in the autumn, it will instantly vegetate. This effect appears to me to arise from the latter plant's possessing a degree of irritability, which has been exhausted in the former, by the heat of the stove, but which it will acquire again during the winter, or by being drawn out, and exposed for a short time to the autumnal frost. On the same principle, we may point out the cause why seedling plants always thrive better in the spring than in the autumn, though the weather be apparently less favourable. In the former season, the stimulus of heat and light is gradually becoming greater than that to which the plant has been accustomed; in the latter season, it becomes gradually less.

There is another circumstance attending trees that have been made to blossom early in the preceding spring, which has always appeared to me an extremely interesting one. If a peach-tree, for example, be brought into blossom in one season in the beginning of February, by artificial heat, it will spontaneously shew strong marks of vegetation at the approach of that season in the succeeding year; and, if it be not well protected, it will expose its blossoms to almost inevitable destruction. I do not see any cause to which this effect can be attributed, except to the accumulated irritability of the plant.

That heat is the remote cause of the ascent of the sap, cannot I think be doubted; and perhaps frequent variations of it are,

in some degree, requisite; (for plants have always appeared to me to thrive best with moderate variations of temperature;) but the immediate cause will, I think, be found in an intrinsic power of producing motion, inherent in vegetable life; and I hope to be able to point out an agent, by which the mechanical force required may possibly be given.

There is, you know, in every kind of wood, what workmen call its grain, consisting of two kinds, the false or bastard, and the true or silver grain. The former consists of those concentric circles which mark the annual increase of the tree; and the latter is composed of thin laminæ, diverging in every direction from the medulla to the bark, having little adhesion to each other at any time, and less during the spring and summer, than in the autumn and winter; whence the greater brittleness of wood in the former seasons. These laminæ (which are of different width in different kinds of wood) lie between, and press on, the sap-vessels of the alburnum: they are visible in every wood that I have had an opportunity to examine, except some of the palm tribe; and these appear to me to have peculiar organs, to answer a similar purpose. If you will examine a piece of oak, you will find the laminæ I describe; and that every tube is touched by them at short distances, and slightly diverted from its course. If these are expansible under changes of temperature, or from any cause arising from the powers of vegetable life, I conceive that they are as well placed as is possible, to propel the sap to the extremities of the branches; and their restless temper, after the tree has ceased to live, inclines me to believe, that they are not made to be idle whilst it continues alive.

I shall at present confine my observations to the English oak, though the same are applicable, in a greater or less degree,

to every other kind of wood. In sawing this tree into boards, it is usual to cut it, as much as possible, into what are called quarter boards; which are so named because the tree is first cut into quarters. In a perfect board of this kind, the saw exactly follows the direction in which the tree most readily divides when cloven: in this case, the laminæ of the silver grain lie parallel with the surface of the board; and a board thus cut, when properly laid in the floor, is rarely or never seen to deviate from its true horizontal position. If, on the contrary, one be sawed across the silver grain, it will, during many years, be incapable of bearing changes of temperature, and of moisture, without being warped; nor will the strength of numerous nails be sufficient entirely to prevent the inconvenience thence arising. That surface, of a board of this kind, which grew nearest the centre of the tree, will always shew a tendency to become convex, and the opposite one concave, if placed in a situation where both sides are equally exposed to heat and moisture. You may probably have observed, that when an oak has been deprived of its bark, and exposed to the sun and air, its surface has been every where covered with small clefts. These are always formed by the laminæ of the silver grain having parted from each other; and they will long continue to open and close again with the changes of the weather. In the last summer, I very frequently placed pieces of oak, recently deprived of its bark, in a situation where it was fully exposed to the sun, but defended from rain. The surface of the tree, in a few hours, presented a great number of small clefts, into which I put, in the middle of the day, the points of small iron pins. Examining these late in the evening, I found that the wood closed so much as to hold them firmly; and, early in the next morning, they were not easily

withdrawn; but, as the influence of the sun increased, the clefts again gradually opened, as in the preceding day, and the pins always dropped out. I could never discover that any weight was gained by the wood during the night; but I was not provided with a balance of proper sensibility to ascertain this point. This experiment was frequently repeated, and always with precisely the same result. After long exposure to the air and light, the wood loses this property.

If the motion I have supposed the silver grain to possess, in the living tree, be more than you think can properly be admitted to belong to vegetable life, I will request your attention to the power of moving in the vine-leaf, on which I have made many experiments. It is well known that this organ always places itself so that the light falls on its upper surface; and that, if moved from that position, it will immediately endeavour to regain it; but, the extent of the efforts it will make, I have not any where seen noticed. I have very frequently placed the leaf of a vine in such a position, that the sun has shone strongly on its under surface; and I have afterwards put obstacles in its way, on whichever side it attempted to escape. In this position, the leaf has tried almost every method possible to turn its proper surface to the light; and I have several times seen one which, having tried during several days to approach the light in one direction, and having nearly covered its under surface, by bending its angular points almost to touch each other, has unfolded itself again, and receded farther from the glass, to approach the light in an opposite direction. As the whole effect here produced appears to arise merely from the light falling on the under surface of the leaf, I cannot conceive how the contortions of its stalk, in every direction, can be accounted for, without admitting, not

only that the plant possesses an intrinsic power of moving, but that it also possesses some vehicle of irritation; and, without this, it will I think be difficult to explain how the heat applied to the branch of the vine, within the stove, can put the sap in the roots and external stem into motion. It may be objected, that these are always ready, when the branch calls for nourishment; and that they are no way affected by the internal heat. But this I cannot admit to be the case; because I have found that the stem suddenly becomes extremely susceptible of injury from cold, as soon as the branch begins to vegetate; and that its whole powers will be paralyzed for some days, by exposure for a few hours to a freezing temperature.

I have had very frequent opportunities of observing a remarkable power in trees, of transferring their sap from one tube to another; for I have often intersected, in the trunk, every tube which led to a lateral branch, and still this branch has derived a considerable portion of nourishment from the trunk. And, if the tubes of an annual shoot of the oak be traced downwards in the autumn, they will be found to pass along the layer of wood of the preceding summer, without any apparent communication between them and the tubes of any former year's growth. Yet the sap rises through the whole of the white wood; and it must be transferred from the internal tubes to those near the surface, which alone appear to communicate with the central tubes of the young shoots and leaves. Indeed we have frequent evidence that trees possess this power; for we see that the whole sap of the stock is carried into an inserted bud or graft.

I at one time suspected, that a small portion of sap, in its

descent from the leaves, had been carried down by the wood, through my incisions, in the preceding experiments on the crab-tree; because I observed a very small increase in size, in the lower part of the stocks; which, I think, could not have taken place without some matter derived from the leaves. But subsequent observation induces me to believe, that the small quantity of additional matter found in the lower part of the stock came from a different source. In those experiments, I paid little attention to any small shoots which sprang from the trunk at some distance below the incisions; and the buds which usually began to vegetate about Midsummer, were not always rubbed off, till some minute leaves appeared. Through these, I now believe that a small quantity of sap was thrown into the bark, and carried up through its tubes, by capillary attraction, when the current from above was intercepted. For the increase of size in the stock always diminished, as it ascended towards the incision; which, I think, would not have been the case, had it been produced by nourishment descending from the upper parts of the tree.

Nothing has occurred, in the preceding experiments, to throw much light on the office of the medulla, to which LINNÆUS and subsequent writers have annexed so much importance; but I will now endeavour to point out one of its offices. In the young and succulent shoot, this substance is extremely full of moisture; and, as there is an immediate communication between it and the leaf, through the central tubes, I conclude it forms a reservoir, to supply the leaf with moisture, whenever an excess of perspiration puts that in a state to require it. Some reservoir of this kind appears to me to be necessary to plants; for their

young leaves are excessively tender, and they perspire much, and cannot, like animals, fly to the shade and the brook. In the mature annual branches, and in those of more than one year old, the medulla is dry, and, I think, it is evidently lifeless; but the space it occupies is never filled with wood, as some naturalists have imagined.

The heart or coloured wood, distinguished from the alburnum, seems to execute an office somewhat similar to the bone in the animal economy. The rigid texture of the vegetable fibre, has rendered this substance unnecessary in the young subject; but, as the powers of destruction, both from winds and gravity, increase in a compound ratio with the growth of the tree, some stronger substance than the alburnum may be supposed to be wanting, to support the additional weight of fruit and seeds. In the root, this substance cannot be wanted, and there it is not found; but, if the mould be taken away from the roots round the trunk, so that they are exposed to the air, and made to support the weight of the tree, they become as full of coloured wood as the trunk and large branches. Having cut through the alburnum of an oak all round, not the slightest mark of vegetation appeared in the succeeding spring; and, having been unable to impel either air or water through its tubes, I conclude that the coloured wood of the oak is without circulation: I see very little reason, however, to admit that it is without life, in a young or middle aged tree. The new matter which enters into the internal part of the alburnum, on its conversion into heart or coloured wood, seems to be of a nature different from the alburnum itself; for it not only changes its colour, which is nearly white, to a dark brown, but it renders it at least ten times

more durable. Some portion of this increased durability may, perhaps, be attributable to the superior solidity of the coloured wood; but, a little attention to the common kinds of English timber, (omitting the resinous tribe,) will convince us that these qualities, though frequently found together, have very little connection with each other. If a number of oaks of the same age be examined, it will be found that, in some individuals, the alburnum consists of a greater number of annual layers than in others, and that the coloured wood will have approached nearer the bark on one side than on the other, in the same tree; the termination also of the coloured wood, and the commencement of the alburnum, are often found in the middle of an annual layer of wood; and each substance, at the points of contact, possesses all its characteristic properties. The alburnum, I think, evidently extends itself laterally, without any radicles descending from the leaves or buds above. I have often procured an union, by grafting, between trees of different kinds, and have sometimes found mere varieties of the same species of tree, whose wood was sufficiently distinguishable, in every stage of future growth, to allow me readily to trace their line of union. The wood of the graft, does not at all descend below its original place of junction with that of the stock; which, immediately below, wholly retains its native character; and, in the part where both are spliced together, each constantly extends itself in the direction of the divergent laminæ of its silver grain. The heart-wood also appears to increase by lateral extension; but I am ignorant of the channels through which the additional matter is conveyed to it.

I will now take the liberty to trespass on your patience, by

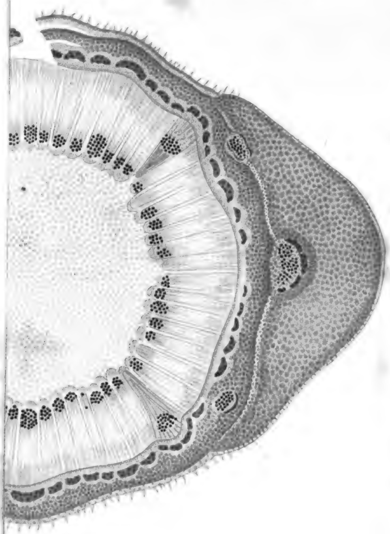
stating a few of the conclusions that I have ventured to draw from the foregoing, and many similar experiments. As I have not been able to find the spiral tubes any where, except immediately surrounding the medulla in different parts, in the seed, and in the leaf, and as they every where terminate at short distances, I conclude that the sap is not raised by their agency; nor by the central vessels, to which they are appendages; for these extend no greater length downwards than the spiral tubes, and terminate with them, at the external surface of that annual layer of wood to which they belong; and they have not any apparent communication with the similar vessels of the succeeding year. In the lower parts of hollow trees, they must long have ceased to exist at all; and, in all trees, except very young ones, they are (as it were) ossified within the heart-wood; and those in the annual shoots and buds are often a hundred and fifty feet distant from the roots, from which they are supposed to raise the sap.

The common tubes of the alburnum, (which do not appear to me to have been properly distinguished from the central vessels, by the authors that I have read,) extend from the points of the annual shoots to the extremities of the roots; and up these tubes the sap most certainly ascends, impelled, I believe, by the agency of the silver grain. At the base of the buds, and in the soft and succulent part of the annual shoot, the alburnum, with the silver grain, ceases to act, and to exist; and here, I believe, commences the action of the central vessels, with their appendages, the spiral tubes. By these, the sap is carried into the leaves, and exposed to the air and light; and here it seems to acquire (by what means I shall not attempt to decide) the power to generate the various inflammable substances that are

found in the plant. It appears to be then brought back again, through the vessels of the leaf stalk, to the bark, and by that to be conveyed to every part of the tree, to add new matter, and to compose its various organs for the succeeding season. When I have intentionally shaded the leaves, I have found that the quantity of alburnum deposited has been extremely small.

In speaking of the circulation within the apple and pear, I wish to express myself with much less decision, as I have not seen the effects of taking up any of those vessels into which the coloured infusions did not enter. The internal organization of the leaf, and of the wood, of those trees which have a central medulla, seems to admit but of little variation, and (as far as I have had opportunities to examine) of no essential difference; whilst that of different fruits is extremely various. The external vascular parts of the apple and pear, abstracted from those which seem to carry nourishment to the seeds, appear to me to resemble, in some respects, those of the leaf; and, relative to the offspring, I suspect that they perform a somewhat similar office.

I do not know how much you will have found in the preceding narrative, that is new and interesting to you, for I am not very deeply read in the experiments which naturalists have made on plants. In the authors I have looked into, I have seen many contradictory experiments related, and many conclusions drawn from a small number of facts; and I have found much that does not well agree with the things that have come under my own observation. I will therefore venture to indulge the hope, that you will have found enough that is new, to



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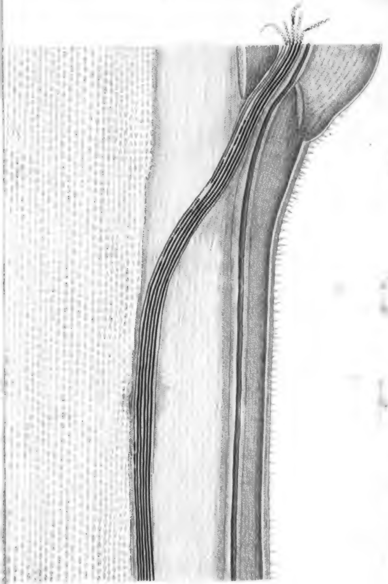
Philos. Trans. MDCCCI. *Plate* XXVI. *p.* 352.



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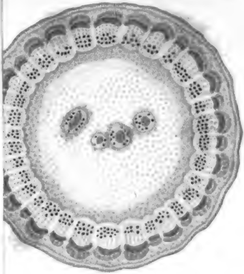

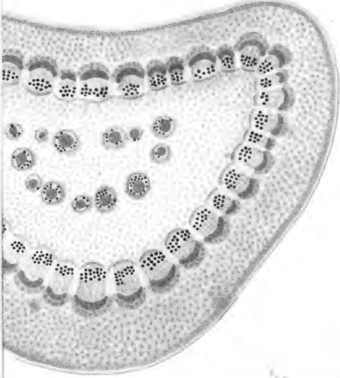

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serve as an apology for me, for having taken up so much of your time.

I have the honour to be, &c.

T. A. KNIGHT.

Elton, Feb. 22, 1801.

XVI. *Additional Observations tending to investigate the Symptoms of the variable Emission of the Light and Heat of the Sun; with Trials to set aside darkening Glasses, by transmitting the Solar Rays through Liquids; and a few Remarks to remove Objections that might be made against some of the Arguments contained in the former Paper.* *By* William Herschel, *LL. D. F. R. S.*

Read May 14, 1801.

HAVING brought up the solar observations, relating to the symptoms of a copious emission of the light and heat of the sun, to the 2d of March, I give them continued in this Paper to the 3d of May. It will be seen, that my expectations of the continuance of the symptoms which I supposed favourable to such emissions, have hitherto been sufficiently verified; and, by comparing the phænomena I have reported, with the corresponding mildness of the season, my arguments will receive a considerable support.

I have given the following observations without delay, as containing an outline of the method we ought to pursue, in order to establish the principles which have been pointed out in my former Paper. But we need not in future be at a loss how to come at the truth of the current temperature of this climate, as the thermometrical observations, which are now regularly published in the Philosophical Transactions, can furnish us with a proper standard, with which the solar phænomena may be compared. This leads me to remark, that, although I

have, in my first Paper, sufficiently noticed the want of a proper criterion for ascertaining the temperature of the early periods where the sun has been recorded to have been without spots, and have also referred to future observations for shewing whether a due distribution of dry and wet weather, with other circumstances which are known to favour the vegetation of corn, do or do not require a certain regular emission of the solar beams, yet, I might still have added, that the actual object we have in view, is perfectly independent of the result of any observations that may hereafter be made, on the favourable or defective vegetation of grain in this or in any other climate. For, if the thermometer, which will be our future criterion, should establish the symptoms we have assigned, of a defective or copious emission of the solar rays, or even help us to fix on different ones, as more likely to point out the end we have in view, we may leave it entirely to others, to determine the use to which a fore-knowledge of the probable temperature of an approaching summer, or winter, or perhaps of both, may be applied; but still it may be hoped that some advantage may be derived, even in agricultural economy, from an improved knowledge of the nature of the sun, and of the causes, or symptoms, of its emitting light and heat more or less copiously.

Before I proceed, I must hint to those who may be willing to attend to this subject, that I have a strong suspicion that one half of our sun is less favourable to a copious emission of rays than the other; and that its variable lustre may possibly appear to other solar systems, as irregular periodical stars are seen by us; but, whether this arises from some permanent construction of the solar surface, or is merely an accidental circumstance,

must be left to future investigation: it should, however, be carefully attended to.

OBSERVATIONS OF THE SUN.

March 4, 1801. I viewed the sun with a skeleton eye-piece, into the vacancy of which may be placed a moveable trough, shut up at the ends with well-polished plain glasses, so that the sun's rays may be made to pass through any liquid contained in the trough, before they come to the eye-glass.*

Through spirit of wine, I saw the sun very distinctly. There are 10 openings without shallows; and a pretty considerable one with a shallow. The opening is nearly round; and the shallow is concentric with it, and also round. The want of shallows about the small openings, and the roundness of that about the largest, indicate that the elastic empyreal gas which passes through them, is without side-bias in its motion.†

March 8. I viewed the sun through water. It keeps the heat off so well, that we may look for any length of time, without the least inconvenience. There are a few openings, many ridges and nodules.

March 9. The ridges near the preceding limb are more extensive than I have ever seen them; there is a broad zone of them.

March 12. There is a cluster of 20 small openings; none of them have any shallows.

March 13. There are 31 openings in the cluster of yesterday:

* See Plate XXVIII. Fig. 1 and 2.

† See page 281 of my last Paper, "*Probable Cause of Shallows*;" and page 301, "*The solar Atmosphere, like ours,*" &c.

they are contained in a double row, nearly parallel to the sun's equatorial motion; the largest of them has now a shallow of a considerable size, on its north following side.

The number of small openings near each other, indicates a perpendicular ascent of the empyreal gas that breaks through the atmospheric clouds; and their want of shallows shews the same thing.

March 15. The set of openings which began to enter on the 8th, consists now of 29. There are 3 other small openings in different parts of the sun.

March 16. There is an opening lately entered. The cluster of yesterday has undergone considerable changes.

March 18. The opening of the 16th consists now of 8 different ones; none of them have any shallows.

The whole space about the cluster of the 8th, is surrounded with luminous ridges in many directions.

The corrugations all over the sun are beautiful, and coarse; resembling small nodules joined together like irregular honeycomb.

In a multitude of places, the corrugations are quite detached, like luminous wisps, or slender tufts, standing upright.

March 19. Another set of ridges has entered the disk; it contains one opening.

The corrugations are rich, and may be called luminous wisps, being much disjoined, except at their bottom; they are so rich, that they partake of the yellowish colour of the ridges.

The northern ridges extend a good way into the disk, like a zone.

March 21. There are five sets, containing 29 openings, none of which have any shallows.

At equal distances from the limb, the corrugations are equally coarse all over the disk of the sun.

March 22. An additional opening, with surrounding ridges, has lately entered the north-following limb. I counted 21 openings.

March 31. An opening very near the preceding limb is surrounded by a shallow, which is bordered by a luminous ridge all round it. The opening itself is also bordered by an elevated edge, which is nearly as high as the general surface of the corrugations; but not so high as that which borders the shallow, and stands above the general surface.

April 1, $11^h\ 30'$. I saw the opening of yesterday go out of the limb: it was the only one left.

$2^h\ 0'$. The sun is now without any openings; but the corrugations are very luminous and rich.

April 2. A considerable opening has entered the disk, accompanied with ridges. From its present situation, I conclude it must have entered not long after my last observation yesterday.

The sun is very rich in luminous corrugations, interspersed with bright nodules towards the south pole.

April 4. There are 4 considerable openings, and many ridges, as well as nodules, on the south and north preceding and following limbs.

The north-preceding ridges extend into the sun, till I can no longer distinguish them; and begin again at the north-following side, as far as they generally can be seen from the limb; so that there is probably a whole zone across the disk. Where I lose them, they are generally converted into tufted, rich, coarse corrugations, such as the sun is now every where covered with.

April 6. There are many ridges and rich corrugations; but I

can perceive no opening. The air is not clear enough to discover very small ones.

April 8. A cluster of 7 small openings is visible; and many ridges.

April 10. Five sets contain 32 openings. The sun is full of rich tufted corrugations.

April 17. Two sets of openings contain 20 of them.

April 19. I count 45 openings. The corrugations are extremely rich. The whole solar surface seems to be studded with nodules. There are probably two belts of ridges across the sun's disk; for, on the preceding side, as well as on the following, I see two ends of belts of ridges very plainly, extending over all the space where these phænomena can be seen.

April 20. The whole surface of the sun is rich: the corrugations are tufted. I count more than 50 openings; many of them have considerable shallows about them.

April 23, 6^h. There are above 60 openings in the sun. The last set is much towards the sun's north pole; very rich in ridges, and disturbed neighbouring surface.

April 24. I count above 50 openings. The corrugations seem to be closer than they were yesterday.

April 26. I viewed the sun through Port wine, and without smoke on the darkening glasses; but distinctness was much injured.

April 27. I count 39 openings. Many ridges and rich corrugations.

April 29. Six different sets contain 24 openings. There are many sets of ridges and rich corrugations.

4^h. I viewed the sun through a mixture of ink diluted with water, and filtred through paper. It gave an image of the sun

as white as snow; and I saw objects very distinctly, without darkening glasses.

As one of the largest openings had a considerable shallow, I found, in viewing it through this mixture, that the difference between what I suppose to be the light reflected from opaque, and the direct light of empyreal clouds, is now more striking than I ever had observed it before.

The ridges, through this composition, appear whiter than the rest of the sun.

The tops of the corrugations are whiter than their indentations, instead of approaching to a yellowish cast, as they do in my former way of seeing through green smoked glasses.

The corrugations are very small and contracted to day.

Suspecting that this new way of seeing might represent objects less than they appear, when I view them through an eye-piece that gives them in the manner I have been used to see them, I put on again the former composition; but found the corrugations as small and close then as they appeared before.

I count 36 openings.

When the ink mixture is more diluted, the sun's image will become tinged with purple.

A solution of green vitriol, with a sufficient number of drops of the tincture of galls to stop as much light as is required, gives a dark blue colour to the sun; and, by dilution with water, a light blue. It is considerably distinct.

With this composition, the corrugations look whiter at the top than in their indentations.

The tincture of galls, with as many drops of the solution of green vitriol as will turn it sufficiently black to stop light, makes

the sun look of a deep red colour; and, by dilution, the red will be paler. This composition is not so distinct as the former.

May 2. 5^h 20′. There are 36 openings, contained in six sets.

As I have remarked, March 19th, April 4th, and April 19th, that ridges are generally placed in equatorial zones, so I now may add, that the different sets of openings have also been generally arranged in the same directions.

May 3. 11^h 56′. Ink mixture. There are 37 openings, arranged in two zones. Four sets in the southern zone contain 27, and three sets in the northern have 10 openings. Through this mixture, I can observe the sun in the meridian, for any length of time, without danger to the eye or to the glasses, with a mirror of nine inches in diameter, and with the eye-pieces open, as they are used for night observations.

Slough, May 4, 1801.

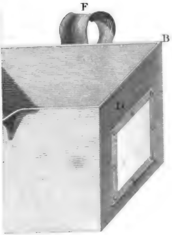
EXPLANATION OF THE FIGURES. PLATE XXVIII.

A B, Fig. 1, is a square trough, closed at the two opposite ends C D, by well polished plain glasses. It will hold any liquid through which the sun's rays are to be transmitted. E is a small spout, and F a handle, so that any portion of the liquid may conveniently be poured out, when the rest is to be diluted.

The trough is made to fit into the open part of the skeleton eye-tube, Fig. 2, resting on the bottom G, and being held in its proper situation by the sides H and I. The end K, at the time of observation, is put into a short tube fixed to the NEWTONIAN telescope, and may be turned about, so as always to have the open part H I horizontal.

When the eye-piece Fig. 3, is screwed, by its end M, into the skeleton tube at L, Fig. 2, and the trough Fig. 1, with any liquid to be tried, is placed in the open part G H I, the sun's rays will come from the small mirror of the telescope to K, and, passing through the plain glasses C D, inclosing the liquid, will enter the eye-piece M, and, after the necessary refractions, come to the eye at N.

Any other, single or double, eye-pieces, of different magnifying powers, may be screwed into L, instead of the piece Fig. 3; and the liquid may easily be tempered so as to intercept a proper quantity of light to suit every eye-glass which is in use, and thus to render the inspection of the sun perfectly convenient.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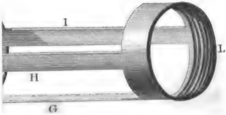
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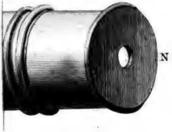
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XVII. *On an improved Reflecting Circle.* *By* Joseph de Mendoza Rios, *Esq. F. R. S.*

Read June 4, 1801.

In Practical Astronomy large instruments are useful, not only to enable the observer to read the angles to a small fraction of a degree, but likewise to diminish, in the construction, the inaccuracies which proceed both from the errors of the divisions and the eccentricity of the index. Frames of considerable dimensions admit also the application of telescopes with great magnifying powers, which is a circumstance of the utmost importance in celestial observations. As the reflecting instruments employed at sea are supported by the hand, their weight and scale are limited within a narrow compass; and it seemed very difficult to obviate, by any expedient, the inconveniences arising from the smallness of their size, while it was impossible to increase it. The celebrated Tobias Mayer contrived, however, a method to determine, at one reading, instead of the simple angle observed, a multiple of the same angle; and, by this means, the instrument became, in practice, capable of any degree of accuracy, as far as regards the above mentioned errors. His invention is essentially different from the mere repetition of the observations; and my object requires that I should explain the principle upon which it is founded.

Mr. Mayer proposed to complete the limb of the Sextant, making a whole Circle, with the horizon glass moveable round

the centre, with an additional index, which I shall call *the horizon index*, in order to distinguish it from *the centre index*, to which the centre glass is attached. This instrument is represented in Plate XXIX. Fig. 1; and the manner of using it is as follows. After the index A is set at o, (the beginning of the divisions,) the two glasses are rendered parallel, as is usually practised with HADLEY's Quadrant, by moving the horizon index B, till the horizon of the sea, (or the sun, or any other object,) or its direct image, and the doubly reflected image of the same, seen through the telescope, coincide. After fixing the horizon index in that position, the centre index A is to be moved, in order to measure the distance of the two objects S and L, (which I shall suppose the sun and moon,) by bringing into contact the doubly reflected image of the sun with the direct image of the moon, seen through the telescope. The centre index will then be at M, and the arch o M might give, as in the Sextant, the angular distance required; but the construction of the Circle renders it easy, in this position, to effect again the parallelism of the glasses, and to make another observation of the contact, in the like manner as from o; which operation will bring the centre index to N. The index will then give o N, or double the distance; and, as it must be divided by 2, in order to have the angle required, the errors of division and eccentricity, which, together, I shall call the *error of the instrument*, will be likewise reduced to one half. It is obvious, that by successive repetitions of the same process, triple, quadruple, &c. the distance may be obtained, and the said error further reduced, in the inverse ratio of the multiplication of the distance, to any degree of approximation required.

The method of rendering the glasses parallel, by means of the

horizon of the sea, is not accurate, on account of the indistinctness of the images; and, when the sun is used for that purpose, the observation becomes fatiguing to the eye. The repetition of that operation, by one or the other method, remained therefore a considerable inconvenience attached to Mr. MAYER's Circle. The author himself seems to have been of that opinion, as he proposed to provide the instrument with a diagonal rule, fixed upon one of the indexes, so that the other index should touch it when the glasses were parallel; but an adjustment of this nature must be subject to great errors, and was never adopted in practice. The Chevalier DE BORDA, wishing to remove that imperfection, had the happy idea of rendering the parallelism of the glasses unnecessary, by substituting the observation of the angular distance of the two objects, to that of the coincidence of the images of the same object. This constitutes the second great improvement of the Reflecting Circle, which it is necessary for me to explain, before I proceed to the account of my own investigations.*

In BORDA's Circle, the telescope is fixed at some distance from the centre, and the horizon glass is carried near the border of the instrument, as in Plate XXIX. Fig. 2. By this arrangement, the rays of light can arrive at the centre glass, both from the heavenly bodies situated to the right of the horizon index, as S′, and from those situated to the left, as S. Thus, if the glasses are parallel to one another, when the centre index is at o, it is obvious that there are two ways of making the observations. While the

* It does not belong to my present plan, to explain the effect of BORDA's improvement in correcting the errors which arise from the want of parallelism in the surfaces of the glasses. This will be fully considered in another Paper, where I intend to give an account of several investigations which I have made upon that particular subject.

direct image of the moon L is seen through the telescope, the angular distance to the sun, if at S, may be measured by moving the centre index to *m*, in order to produce the contact; or, if the sun is at S′, the same operation may be performed, by using the contrary motion to *m′*. The first kind of observation, the Chevalier DE BORDA calls *observation to the left;* and the second, *observation to the right.* Suppose now, that (the horizon index being fixed in the same position) the distance from L to S is observed to the left, by bringing into contact the doubly reflected image of S with the image of L, seen without reflection; let us then turn the instrument round, keeping it in the same plane, so as to have the direct image of S through the telescope, and thus make an observation of the same distance to the right; the position of the centre index being in the first observation at *m*, and in the second observation at *m′*, it is clear, that if o is the point where the parallelism of the glasses takes place, o *m* is equal to o *m′;* and, that the arch *m m′*, determined by the two positions, will give double the distance.

It will be more convenient to have the centre index at o, when the first observation is made, in order to take the double distance at one reading, after the second observation. For this purpose, the first part of the process may be inverted, by previously fixing the centre index at the beginning of the divisions, and moving the horizon index H towards o, instead of moving the centre index A to *m*, or towards H.

The last kind of observation, in which the incident ray, which produces the first image upon the centre glass, may be conceived to run double the angular distance, passing in its way over the line of collimation, has been called, by the Chevalier DE BORDA, *the crossed observation.*

The same process may be repeated, by fixing alternately one of the indexes, and moving the other, and continuing successive sets of observations; each set of two crossed observations, one to the right and another to the left. The angle given by the instrument, will be equal to double the angular distance multiplied by the number of sets observed, or, in other terms, to the angular distance multiplied by the number of observations, which are always supposed to be made by pairs; an odd observation being of no value in this manner of using the Circle.

I have expressed myself as if the observations could always be made by looking alternately at each object through the telescope, in order to bring into contact the doubly reflected image of the other object. This is not the case in the observations of the distances from the moon to the sun, or a star; it being then indispensable to compare, by reflection, the brightest of the two heavenly bodies; but there is a very easy method of obviating that inconvenience. After the contact of the images of S and L is observed, with the telescope directed to L, the position of the plane of the instrument may be inverted, turning it round the axis of vision OBL; the incident ray will then answer to the point S′, equally distant from L as S, and the crossed observations will still give SS′, or double the distance.

Whether a Circle is used simply, as MAYER proposed it, or according to BORDA's method, its peculiar advantages chiefly depend on the multiplication of the distance required. I have therefore turned my attention to the improvement of this principle; and, with that view, I have contrived the construction which I am going to describe.

In the crossed observations made with BORDA's Circle, the indexes move alternately through an arch which, in the divisions,

is equal to double the distance: for example, the centre index comes, in the first crossed observation, from *m* to *m′*; in the third crossed observation, from *m′* to *m″*, &c. and the horizon index, in the second crossed observation, to *b′*; in the fourth crossed observation, to *b″*, &c. and, by each of the two indexes may be found the same multiple of the distance required. Let us now place the Nonius in a circle moving round the centre, over, or adjacent to, the usual limb which contains the divisions: it will easily be conceived, that, by attaching that circle, which I shall call the *Flying Nonius*, alternately to each of the indexes, it will serve as Nonius for both; and that, after any number of observations, it will give the compound motion of the two indexes. Thus, after the first observation, the Flying Nonius will, at each crossed observation, advance double the distance over the divisions, while each separate Nonius, fixed on the indexes, requires a set of two observations, to produce the same effect in BORDA's Circle.

Plates XXX. XXXI. and XXXII. exhibit a perspective view, a plan, and a section, of the instrument, which, for the sake of distinction, I shall call my *Improved Reflecting Circle.* The last Plate is particularly intended to shew the compound handle, which I have adapted to the instrument, in order to hold it with convenience and ease in every position.* These three repre-

* The use of MAYER's Circle, or of BORDA's, as constructed till now, with only one handle attached to the centre, is extremely inconvenient in several positions, and particularly when it must be kept inverted downwards during the observation. For this reason, I thought it of importance to contrive such a support as would enable the observer to hold the instrument with the same ease in every direction. This is effected by means of the compound handle, attached to the horizon index, by the brace V and screw X, (Plates XXX. XXXI. and XXXII.) which index turns round the centre with the handle. When Mr. TROUGHTON began to construct this sort of instruments, I

sentations, in which the same parts are marked with the same letters, are sufficient to give an accurate idea of the arrangement of the whole, and make it unnecessary for me to enter now into a minute detail of the mechanism of the apparatus. I therefore shall content myself with adding here only what concerns the general use, and the peculiar properties, of this instrument.

M is the divided limb of the circle, and N the Flying Nonius, (Plates XXX. XXXI. and XXXII.) to each of which the horizon index may be occasionally attached, by means of the clamps D, C; as well as the centre index, by means of the other clamps A, B. The peculiar property of the instrument being that of giving double the distance, I have thought proper to divide the circle into 360 degrees, and not into 720 according to the nature of the Sextant. Thus, after a crossed observation, the reading of the Nonius will, without reduction, exhibit the measure of the simple distance. I have likewise extended the Nonius round the circumference, so that, by the coincidence of two divisions, the number of degrees will appear on the limb, and that of the minutes and seconds on the Flying Nonius. The manner of making the observations with this instrument is as follows.

recommended to him this improvement, which he has adapted to his Reflecting Circle.

I shall observe here, that Mr. TROUGHTON's Circles are not of the kind which I have endeavoured to improve. The scheme of his construction may be said to consist in completing the limb of a Sextant to the whole circumference, and making it capable thereby of performing BORDA's crossed observations, with as many Noniuses as may be attached to the centre glass. But, Mr. TROUGHTON's instrument is deprived of the principle invented by MAYER, for obtaining at one reading a multiple of the distance required; which is the great property of Circles, and, in my opinion, the best means of diminishing their errors.

Adapt the o of the Flying Nonius to 360° of the limb; and then fasten the two clamps A, B, of the centre index* E E, by which the divisions will be kept in the same relative situation. Then, turn round the horizon index FF, and make an observa ion of the distance to the right. The contact must be adjusted by the screw G, the clamp C being fastened. Leave this clamp fastened, and loosen the clamp A; thus turn the instrument, and make a crossed observation to the left, adjusting by means of the screw H, after having fastened the clamp D. At the end of this observation, the Flying Nonius will give the distance. Fasten now the clamp A, and loosen the clamps B and C, leaving the clamp D fastened; then turn the instrument again, and make a crossed observation to the right. At the end of this observation, the Flying Nonius will give double the distance. By successively inverting the use of the clamps, this alternate process may be continued *ad libitum;* and each crossed observation will increase the reading, by an arch equal to the distance.

Let the number of observations be n, and the angular distance D. The arch given by my improved circle will be $= D\ (n - 1)$. In BORDA's Circle, (reducing the divisions of the Sextant to those of the Theodolite I use,) the arch is $= D \times \frac{1}{2} n$; and, either n must be an even number, or the odd observation must be lost. In MAYER's Circle, the arch is $= \frac{1}{2} D \times \frac{1}{2} n$; and the number n, which comprehends the observations for the parallelism of the glasses and those for the distance, must likewise be even. The

* Properly speaking, neither the centre nor the horizon indexes act, in this instrument, as such, both of them being deprived of the Nonius, which is transferred to the flying circle; but, for the sake of perspicuity, I continue the use of those expressions, in order to distinguish the plates or rules which carry the centre and the horizon glasses.

comparison of these expressions, shews at once the relative advantages of the different instruments.

My construction offers considerable advantages, in every manner of using the Circle. If, instead of the crossed observations, it should be wanted to employ the usual practice of rendering the glasses parallel, a multiple of the distance may still be obtained by my instrument, equal to that of the other method. For this purpose, the parallelism of the glasses may be effected, by means of the images of the sun, or the horizon of the sea, moving the index F, while the o of the Nonius is adapted to 360° of the limb, and the two clamps A, B, are fastened. After this, an observation of the distance to the right may be made, with the clamp A fastened, while the clamp B is loose; the clamp D being also fastened, and the clamp C loose; and, at the end of this observation, the Flying Nonius will give an angle, which will be only the half of the distance in my divisions, but which would be equal to the whole distance, if the divisions were according to the Sextant. After that, and while the clamps B and C are fastened, and the clamps A and D loose, the parallelism of the glasses may be again effected; and the Nonius will advance the same quantity over the limb. The same addition will take place, by inverting the use of the clamps, and making another observation of the distance. The like alternate process may be continued indefinitely; and the result given by the instrument will, with only one observation more, be the same as that of Borda's method, and double the arch which would be obtained by Mayer's Circle.

Mr. Borda's Circle is liable to a very great inconvenience in practice. Each index advances successively over the limb; and,

in order to facilitate the operation of bringing the images for the contact within the telescope, that author advises to make a preparatory memorandum of the positions which the indexes will nearly occupy, so that they may be set accordingly, previous to each observation. But this method, which is always inconvenient, by night becomes almost impossible. For this reason, I have joined to the horizon index an arch LL, (Plates XXX. and XXXI.) which is divided, both to the right and left, into degrees and minutes of the Sextant, so that, when the glasses are parallel, the centre index coincides with the two first divisions o, o, and occupies the space left blank between them. I have further provided two sliding pieces P, P, which may be adapted to that arch, with a spring sufficient to keep them firm in any situation. Putting each of these pieces upon the arch, so that their ends may coincide with the divisions marking the rough distance to be measured, no more will be required, than to set the centre index alternately against each piece, before the beginning of the successive crossed observation. The clamp may then be fastened, and the remainder of the motion produced by the adjusting screw; as, if necessary, the index will push the sliding piece further, and leave it at the point where the contact was effected.*

The Flying Circle facilitates the use of any number of Noniuses, which may be applied round the whole circumference; but, as the leading principle which I have chiefly had in contemplation, is that of obtaining an accurate result from one reading, I have only used a single Nonius. Two Noniuses,

* The idea of this simple contrivance, was suggested to me by the ingenious Mr. E. TROUGHTON.

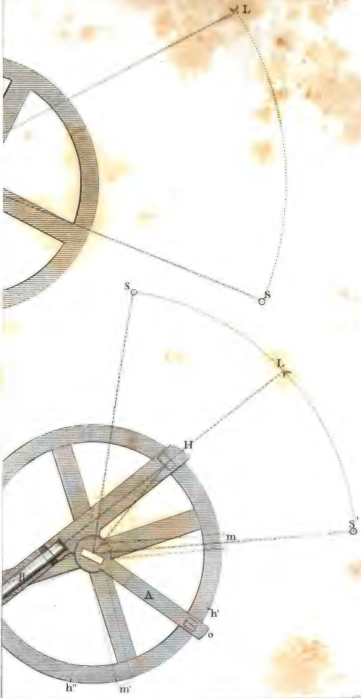
opposite one another, might however be advantageous, in order to correct the errors of eccentricity; but, in my opinion, a greater number ought not, in any case, to be used.

Before I conclude this Paper, I shall remark, that my improvement may be partially applied to a Circle, where the telescope and the horizon glass are attached, or fixed, to the main frame of the instrument. The Flying Nonius, acting then with the single centre index, will only give the same result as BORDA's Circle; but this construction seems to me greatly preferable to all the other plans executed till now; the whole apparatus being more solid and simple, and its use not liable to the errors which arise from the motion of the horizon index.

With this construction, we may likewise employ a method of ascertaining the place where the parallelism of the glasses was observed to take place, and of setting the index afterwards in the same situation, as often as is necessary for the repetition of the observations. A piece may be used, so contrived as to be attached to, or detached from, one side of the index, by means of a screw; and provided besides with other screws, to fasten it to any part of the limb. This rectification piece, being previously attached to, and carried with, the index, must be fixed in the situation it occupied when the contact of the images was observed. The index will then be detached from it, in order to observe the distance, and afterwards must be brought back to the same position as before, contiguous to the rectification piece. The like alternate process may be repeated; and the Flying Nonius, going with the index in the motions forwards, and standing still in the motions backwards, will give the multiple of the observed angle, without performing the observation for the parallelism more than once in the beginning. In MAYER's Circle,

as well as in BORDA's, there is a great objection to any attempt for that purpose; because, as the horizon glass moves round with the index, its perpendicular position is deranged by the inequalities of the plane of the limb; but, in my construction for multiplying with the horizon glass fixed, that inconvenience is removed; and the method of ascertaining the identical position of the glasses may be employed in practice, with advantage, it being done when the index is at the same point of the frame. By suggesting this idea, I do not, however, mean to represent it as preferable to the repetition of the observations; which process must, for many reasons, have the advantage over any mechanical contrivance whatsoever.

I have procured Reflecting Circles to be constructed, upon the principles here described, both with the telescope and the horizon glass upon a moveable index, and with the same pieces attached to the main frame of the instrument. The two methods have respectively answered my expectation; and I purpose, at a future opportunity, to publish a description of the means which I wish to recommend for the mechanical improvement of the different parts, together with an account of some other essays which I have made relative to the same subject.



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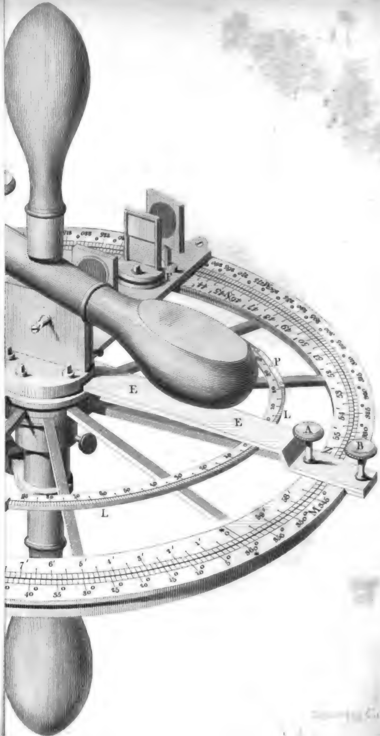
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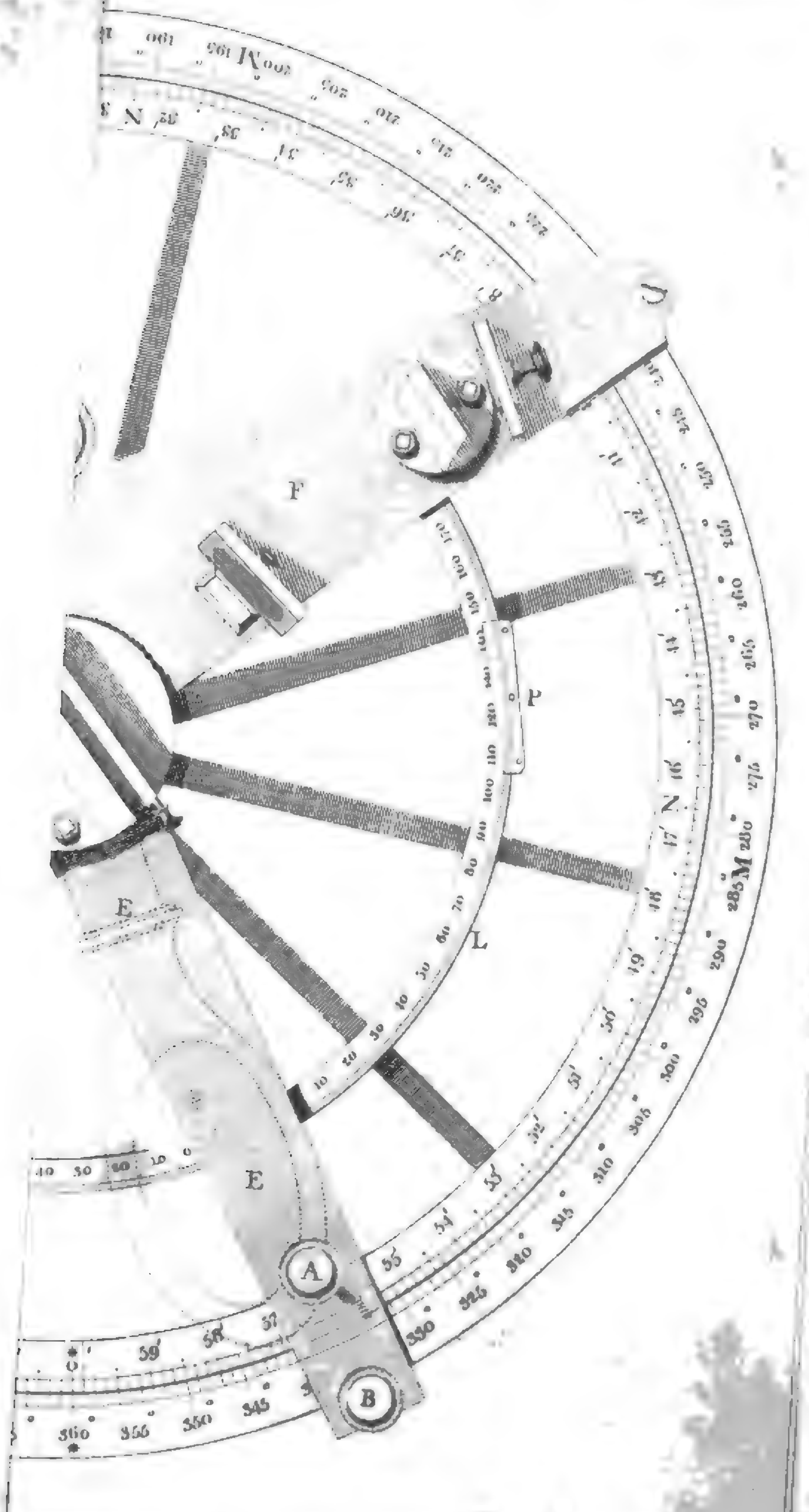

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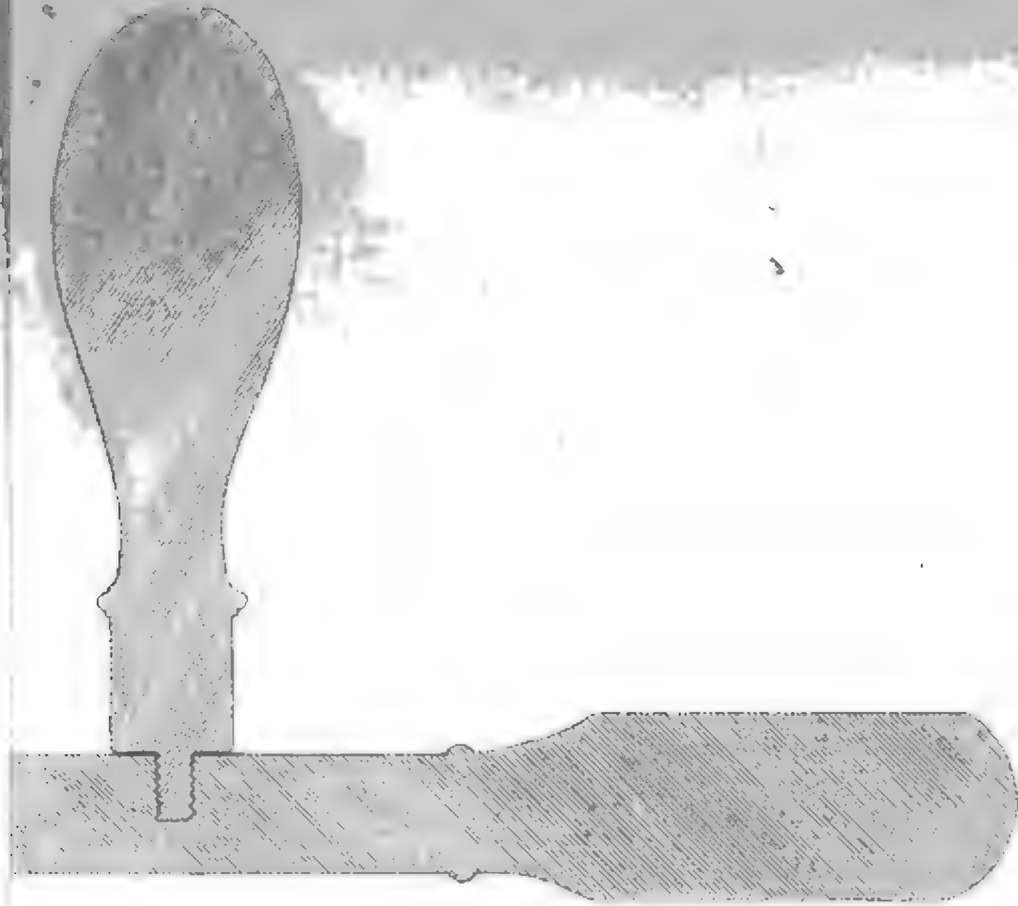

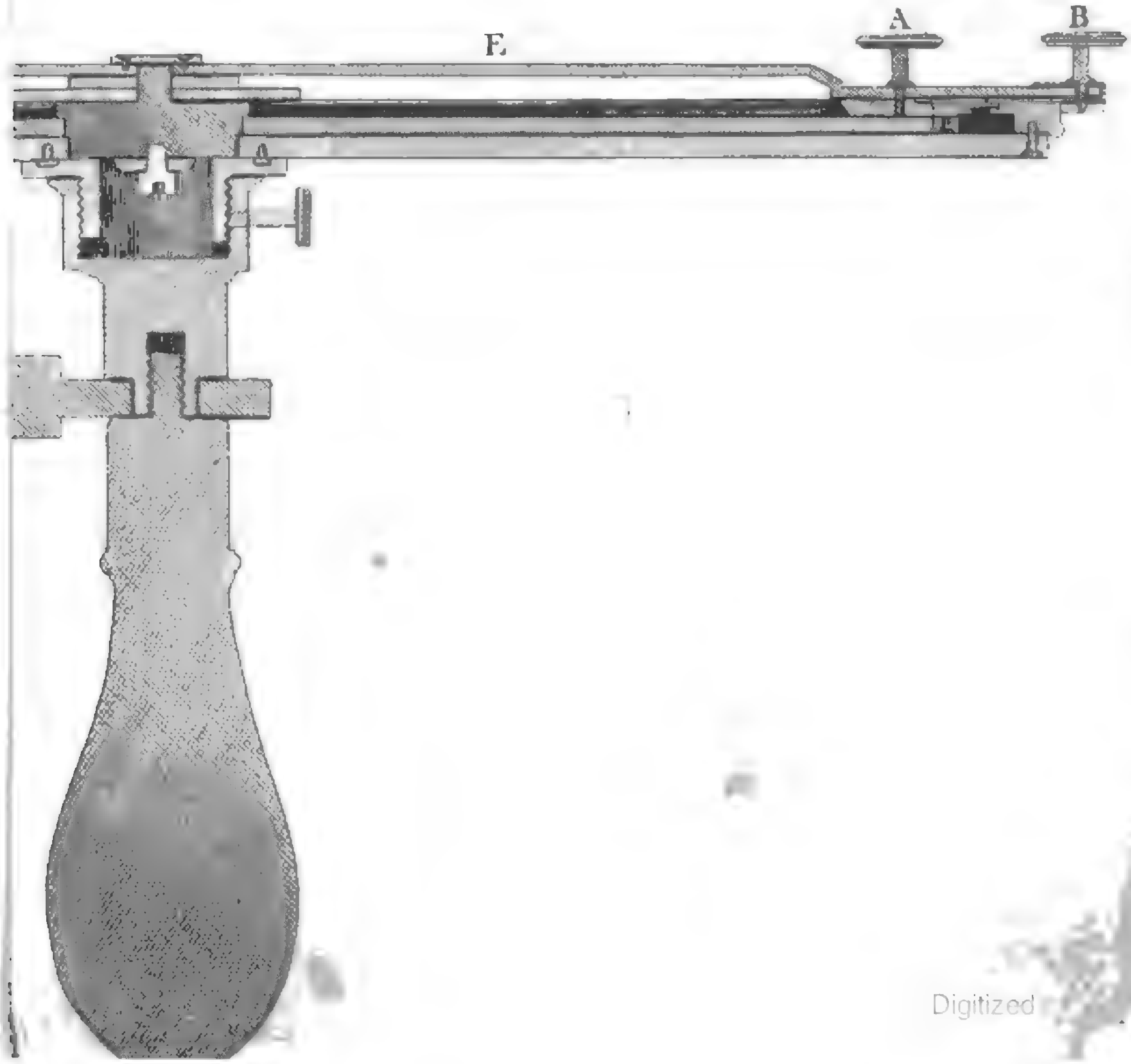
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XVIII. *Observations and Experiments upon Dr.* James's *Powder; with a Method of preparing, in the humid Way, a similar Substance. By* Richard Chenevix, *Esq. F. R. S. M. R. I. A.*

Read June 4. 1801.

After the observations and experiments made by Dr. Pearson, to investigate the nature of Dr. James's Powder, and presented by him to this Society, very little remained to be effected or desired, towards a further knowledge of the subject. But those very experiments served to suggest, that the mode of preparation was far from being the best that the present improved state of chymical knowledge might afford; and that, in all probability, a less defective composition might result from a process more conformable to some improvements, which of late have been advantageously applied to pharmaceutic chymistry.

It may be laid down as a general principle, that, in delicate experiments, whether analytical or synthetical, fire (that potent and once believed to be universal agent) is too precarious in its means, and too uncertain in its application, to be employed with full and constant success. And, if it is still recurred to, the advantage of promptness, and a remnant of ancient custom, are the principal reasons. But, where other methods can be devised to effect the same combinations, (and the humid way offers many,) every person conversant in chymical knowledge will allow the benefit of adopting them. The recent improvement in

the mode of preparing calomel, is a striking example of such salutary corrections being successfully introduced.

A few observations upon the formula according to which Dr. JAMES's Powder, or the *Pulvis Antimonialis*, is prepared, and upon some properties of antimony, will place this assertion in a more prominent point of view.

In order to prepare this powder, we are told to take equal weights of bone or hartshorn shavings and crude antimony, and calcine them together, at a high temperature: in other words, to take phosphate of lime, which already contains a great excess of lime, and add to it an oxide of antimony. In this process, it has been supposed, that the phosphoric acid of the bone or hartshorn will saturate, not only the lime with which it was originally combined, but, in addition to it, a new portion of metallic oxide, and a new portion of lime. For, what little sulphuric acid might, during the process, have been formed by the combustion of the sulphur of the crude antimony, is dissipated, at a much lower temperature than that to which the powder is exposed.

Every oxide of antimony with which we are acquainted, is volatile at a high degree of heat: it would therefore be hazardous to assert, that it is possible to preserve always the same proportion of antimony, whatever care may be employed in directing the operation; and, a dissimilarity in the chemical result, must necessarily be attended with uncertainty in the medical application.

To this property may be added another, no less conducive to error. That portion of oxide of antimony which is not volatilized, becomes, in a great measure, insoluble in all the acids. What the effect of the gastric juice may be, upon a substance

which resists the action even of nitro-muriatic acid, it is not my purpose to determine. It is sufficient for me to say, that, as the quantity of insoluble matter, in a given quantity of Dr. JAMES's Powder, prepared at different times, may vary, the effect of any dose also may differ, according to the proportions of soluble and insoluble matter.

I look upon it as a fortunate circumstance, that those experiments and observations which I mentioned in the beginning of this Paper, existed as a standard to which I might refer my own attempts, and by which I might estimate their validity. Dr. PEARSON has proved, (as by my own experiments I have found,) that in Dr. JAMES's Powder about 28 per cent. resisted the action of every acid. In examining some of the *Pulvis Antimonialis* of the London Pharmacopeia, I found the average quantity of insoluble matter to be about 44 per cent. This proportion, however, was liable to considerable variation.*

The powder here treated of is denominated, by Dr. PEARSON, a triple salt, or a real ternary combination of a double basis, (lime and antimony,) with phosphoric acid. What I have mentioned, with regard to the quantity of acid contained in bone or hartshorn, as being too small to saturate a new portion of these bases, may throw some doubts upon the possibility of any such combination in the present case. But I have made some more direct experiments, which tend to prove, that no such combination does exist.

* I find, from the information of several medical gentlemen, that the *Pulvis Antimonialis* is generally considered as stronger than Dr. JAMES's Powder. This seems rather extraordinary, when we consider that the quantity of insoluble matter is greater in the former than in the latter; and would almost lead us to suspect it to be the active part of the medicine.

I took some white oxide of antimony, (formerly called Algaroth Powder,) precipitated by water from muriate of antimony, and heated it for a long time with phosphoric acid. I decanted the liquor, and washed the powder that remained. No antimony could be found in the liquor; nor could any traces of phosphoric acid be detected in the residuary oxide of antimony. I then took a solution of muriate of antimony, and divided it into two equal parts: into one, I poured distilled water; and, into the other, a solution of phosphate of soda. In each liquor, a copious precipitate was formed; which precipitates, after being well washed, were dried. The weight of both was the same; whereas, it is evident that, had any phosphoric acid been combined with the oxide, there would have been an augmentation of weight, in that which was precipitated by the solution of phosphate of soda. This precipitate likewise, upon examination, gave no traces of phosphoric acid. From these experiments it appears, that there exists no combination, which can be denominated a phosphate of antimony.

To attempt an explanation of the real nature of the powder here spoken of, I had recourse to some experiments of Mons. BERTHOLLET. By detonating sulphuret of antimony and nitrate of potash, in a crucible, he obtained a mass, which he reduced to powder, and washed. The liquor gave, upon evaporation, a crystallized salt, which M. BERTHOLLET terms an *antimoniate of potash*. I never could succeed in any attempt to form a similar combination between the above white oxide of antimony and potash, owing, I believe, to the small quantity of oxygen contained therein, compared with that which is combined with the oxide obtained by detonation. I cannot therefore say, that the

powder in question is, in any degree, what M. BERTHOLLET would call an *antimoniate of lime.*

But, be the state, whether of mixture or of combination, what it may, my purpose is to endeavour to produce a substance, which, from its more certain mode of preparation, may be more equal and constant in its effects.

Dissolve, together or separately, in the least possible portion of muriatic acid, equal parts of the forementioned white oxide of antimony and of phosphate of lime.* Pour this solution gradually into distilled water, previously alkalizated by a sufficient quantity of ammonia. A white and abundant precipitate will take place, which, well washed and dried, is the substitute I propose for Dr. JAMES'S Powder.

The theory of this precipitation is so clear and simple, that it does not require any comment. It may be useful, however, to those who wish to make this preparation, to remark, that it is absolutely necessary that the solution of phosphate of lime and of oxide of antimony, in muriatic acid, should, after being well mixed, be poured *into the alkaline liquor*, in order to obtain a precipitate homogeneous throughout the operation. For, should the alkaline liquor be poured *into the acid*

* In order to procure the phosphate of lime, I dissolved in muriatic acid, a quantity of calcined bone, and precipitated by ammonia, in its state of greatest causticity. By this means, the excess of muriatic acid, which held in solution the phosphate of lime, is saturated, and the phosphate is precipitated; but no muriate of lime is decomposed, if the ammonia is quite free from carbonic acid. This is the most direct method of obtaining phosphate of lime pure. This salt is not decomposed, as some have asserted, by muriatic acid, but merely dissolved by it. I have been induced to state fully these particulars, because, from the beneficial effects of this salt in the treatment of rachitis, as proposed by M. BONHOMME, (*Annales de Chimie*, Vol. XVIII. p. 113,) it may become of general use. The oxide of antimony, I obtained by precipitating, by water, the common butter of antimony of the shops.

solution, the water of the former would act upon the entire mass of oxide of antimony, while the alkali would precipitate the phosphate of lime only as it saturated the acid which held that salt in solution: thus, the precipitate would contain more antimony in the beginning; and, towards the end, the phosphate of lime would be predominant. For the same reason too, a pure alkali is preferable to its carbonate; for the carbonic acid disengaged, would retain in solution a portion of phosphate of lime.

Whether this composition be a chymical combination or a mixture, I will not take upon me to determine; but, for the reasons above mentioned, in speaking of Dr. JAMES's Powder, I believe it to be merely a very intimate mixture. At all events, it must be more homogeneous than any that can be prepared in the dry way. It is entirely soluble in every acid that can dissolve either phosphate of lime or oxide of antimony separately; and, to have it constantly and uniformly the same, no further address in preparing it is required, than to avoid the errors I have mentioned.

As, after some medical trials of the powder, it was suggested to me, that it might be advantageous to render it somewhat stronger, I prepared another portion, by taking two parts of oxide of antimony and but one of phosphate of lime, and precipitating as above described. The medicinal power was then considerably increased.

Dr. JAMES's Powder is a medicine which has been so long in use, and is so deservedly ranked among the most valuable we possess, that every attempt to render the process for preparing it more simple and more certain, must be allowed to be of some importance. But, whatever reason there was to think, by arguing upon its chymical properties, that I had really succeeded

in improving its medicinal virtues, it still remained to be proved, by actual experiment, that the hoped-for success was not merely conjectural. To ascertain this, I gave some of my powder to Dr. CRICHTON, Dr. BABINGTON, and Mr. ABERNETHY; gentlemen whose extensive practice and acknowledged skill sufficiently enabled them to judge of its medical properties. They all concur in opinion, that, in its general effects, it agrees with Dr. JAMES'S Powder and the *Pulvis Antimonialis;* but, that it is more mild, and consequently may be given in larger quantities, seldom producing nausea or vomiting, in doses of less than eight or ten grains.

XIX. *Case of a young Gentleman, who recovered his Sight when seven Years of Age, after having been deprived of it by Cataracts, before he was a Year old; with Remarks. By Mr.* James Ware, *Surgeon. Communicated by* Maxwell Garthshore, *M. D. F. R. S.*

Read June 11, 1801.

Master W. the son of a respectable clergyman, at Castlecary, in Somersetshire, was born in the year 1793; and, for many months, appeared to be a healthy perfect child: his eyes, in particular, were large and rather prominent. When about six months old, he began to cut his teeth; which was attended with great pain, and frequently with violent convulsive fits. About the end of his first year, a number of persons passing in procession near his father's house, accompanied with music and flags, the child was taken to see them; but, instead of looking at the procession, it was observed that, though he was evidently much pleased with the music, his eyes were never directed to the place from whence the sound came. His mother, alarmed by this discovery, was naturally led to try whether he could see silver spoons, and other glaring objects, which she held before him at different distances; and she was soon convinced, that he was unable to perceive any of them. A surgeon in the country was consulted, who, on examining the child's eyes, discovered an opacity in the pupils, which was so considerable, that he did not hesitate to pronounce there was a complete cataract in each.

A description of the child's situation was then sent to me, with a request that I would point out those steps which its parents should pursue. The case was so evident, that I could not hesitate in saying, that the removal of the opaque crystalline humour, from the place it occupied behind the pupil, was the only method by which the child could obtain his sight; and, attached as I was, at that time, in all cases, to the operation of extracting the cataract, in preference to that of depressing it, I added, that I did not think he would be fit for the operation, until he was at least thirteen or fourteen years old. This advice being approved, all thoughts of assisting his sight were, for the present, relinquished. He soon discovered a great fondness for music; his memory was very retentive of the little stories that were read or recited to him; and, in every way, it became evident that he had a mind capable of receiving information. As soon as he could speak, it was also observed, that when an object was held close to his eyes, he was able to distinguish its colour, if strongly marked; but, on no occasion, did he ever notice its outline or figure. In November, 1800, his parents took him to Bristol; whither they went for the purpose of seeing the works carried on in the school for the indigent blind in that city, and in order that they might ascertain whether their son, who was then arrived to his seventh year, could be taught any thing that would be useful or amusing. Here he very quickly learnt the art of making laces. But his parents, having brought him so far from home, thought it adviseable to extend their plan, and make a visit to the metropolis, for the sake of giving me an opportunity of inspecting his eyes, and of hearing whether my opinion continued the same as that which I had written to them six years before. About a month previous to

the time of their arrival, a Portuguese boy, fourteen years old, had been put under my care, who was in a similar situation; and, in this case, notwithstanding all the efforts I could use, I found it impossible to fix the eye, in order to extract the cataract, without employing a degree of force which might have been highly injurious. I therefore relinquished my intention of performing the operation in that way, and determined to make use of the couching needle; being prepared, either to depress the cataract with this instrument, if it was sufficiently solid for the purpose, or, if it was soft or fluid, (which I rather expected,) to puncture its capsule largely, so as to bring the opaque crystalline into free contact with the aqueous and vitreous humours. In order to fix the eye for this operation, I was not afraid to make use of a speculum oculi; since a pressure, which would have been highly dangerous in extracting the cataract, might be applied on the present occasion with perfect safety. Conformably to my expectation, the cataract was of a soft consistence; in consequence of which, I was not able to depress it, and contented myself with making a large aperture through the capsule, by means of which the crystalline was brought into contact with the other humours, a considerable part of it coming forwards, and shewing itself directly under the cornea.

This being the immediate result of the operation, it could not be expected that any improvement should be made in the sight of the patient at that time. In a few days, however, the opaque matter was wholly absorbed; the pupils became clear; and the lad recovered the sight of both his eyes.* Encouraged by the

* It should be remarked, that the sight obtained by children who are born with cataracts, is seldom so perfect as that which those recover, after the operation, who are

success which followed this operation, I was induced to retract the opinion which I had formerly sent to Master W.'s father, (which opinion I had given under the impression that the cataract should be extracted,) and I now proposed, that an attempt should be made to afford relief to one eye, at least, without further loss of time; this attempt, in the way above mentioned, being practicable with as much safety at his present age as at any future period; and, if it proved successful, it would give the young gentleman the benefit of vision five or six years sooner than his friends had been encouraged to expect, by my former letter on this subject. They were naturally much pleased with this alteration in my advice; and the child himself appearing to possess a great degree of fortitude, I performed the operation on the left eye, on the 29th of December last, in the presence of Mr. CHAMBERLAYNE, F. A. S. Doctor BRADLEY, of Baliol College, Oxford, and Mr. PLATT, surgeon, in London. It is not necessary, in this place, to enter into a description of the operation. It will be sufficient to say, that the child, during its performance, neither uttered an exclamation, nor made the smallest motion, either with his head or hands. The eye was immediately bound up, and no inquiries made on that day with regard to his sight. On the 30th, I found that he had experienced a slight sickness on the preceding evening, but had made no complaint of pain, either in his head or eye. On the 31st, as soon as I entered his chamber, the mother, with much joy, informed

afflicted with the disorder later in life. In consequence either of some remaining opacity in the crystalline capsule, which hinders the free admission of the rays of light, or of a greater tenuity in the remaining humours of the eye, children require, in general, a much deeper convex glass to enable them to see minute objects; and, at the same time, they are obliged to hold them much nearer their eyes than older persons.

me that her child could see. About an hour before my visit, he was standing near the fire, with a handkerchief tied loosely over his eyes, when he told her that under the handkerchief, which had slipped upward, he could distinguish the table by the side of which she was sitting: it was about a yard and a half from him; and he observed that it was covered with a green cloth, (which was really the case.) and that it was a little farther off than he was able to reach. No further questions were asked him at that time; as his mother was much alarmed, lest the use thus made of his eye might have been premature and injurious. Upon examination, I found that it was not more inflamed than the other eye; and the opacity in the pupil did not appear to be much diminished. Desirous, however, to ascertain whether he was able to distinguish objects, I held a letter before him, at the distance of about twelve inches, when he told me, after a short hesitation, that it was a piece of paper; that it was square, which he knew by its corners; and that it was longer in one direction than it was in the other. On being desired to point to the corners, he did it with great precision, and readily carried his finger in the line of its longest diameter. I then shewed him a small oblong band-box covered with red leather, which he said was red and square, and pointed at once to its four corners. After this, I placed before him an oval silver box, which he said had a shining appearance; and, presently afterwards, that it was round, because it had not corners. The observation, however, which appeared to me most remarkable, was that which related to a white stone mug; which he first called a white bason, but, soon after, recollecting himself, said it was a mug, because it had a handle. These experiments did not give him any pain; and they were made in the presence of his mother, and of Mr.

Woodford, a clerk in his Majesty's Treasury. I held the objects at different distances from his eye, and inquired very particularly if he was sensible of any difference in their situation; which he always said he was, informing me, on every change, whether they were brought nearer to, or carried further from him. I again inquired, both of his mother and himself, whether he had ever, before this time, distinguished by sight any sort of object; and I was assured by both that he never had, on any occasion; and that, when he wished to discover colours, which he could only do when they were very strong, he had always been obliged to hold the coloured object close to his eye, and a little on one side, to avoid the projection of the nose. No further experiments were made on that day. On the 1st of January, I found that his eye continued quite free both from pain and inflammation, and that he felt no uneasiness on the approach of light. I shewed him a table knife; which at first he called a spoon, but soon rectified the mistake, giving it the right name, and distinguishing the blade from the handle, by pointing to each as he was desired. He afterwards called a yellow pocket-book by its name, taking notice of the silver lock in the cover. I held my hand before him; which he knew, but could not at first tell the number of my fingers, nor distinguish one of them from another. I then held up his own hand, and desired him to remark the difference between his thumb and fingers; after which, he readily pointed out the distinctions in mine also. Dark-coloured and smooth objects, were more agreeable to him than those which were bright and rough. On the 3d of January, he saw, from the drawing-room window, a dancing bear in the street; and distinguished a number of boys that were standing round him, noticing particularly a bundle of clothes which

one of them had on his head. On the same evening, I placed him before a looking glass, and held up his hand: after a little time he smiled, and said he saw the shadow of his hand, as well as that of his head. He could not then distinguish his features; but, on the following day, his mother having again placed him before the glass, he pointed to his eyes, nose, and mouth, and seemed much gratified with the sight.

Having thus stated the principal observations that were made by Master W. I shall now make a brief comparison between this statement, and that which is given in the XXXVth Volume of the Philosophical Transactions, of Mr. CHESELDEN'S patient, who was supposed to be born blind, and obtained his sight when he was between thirteen and fourteen years old.

It should be observed, that though Master W. was six years younger than Mr. CHESELDEN'S patient, he was remarkably intelligent, and gave the most direct and satisfactory answers to every question that was put to him. Both of them, also, if not born blind, lost their sight so very early, that, as Mr. CHESELDEN expresses it, "they had not any recollection of having "ever seen."

My first remark is, that, contrary to the experience of Mr. CHESELDEN'S patient, who is stated "to have been so far from "making any judgment of distance, that he thought all objects "touched his eyes, as what he felt did his skin," Master W. distinguished, as soon as he was able to see, a table, a yard and a half from him; and proved that he had some accuracy in his idea of distance, by saying, that it was a little further off than his hand could reach. This observation, so contrary to the account we have received of Mr. CHESELDEN'S patient, would have surprised me much more than it did, if I had not previously, in

some similar instances, had reason to suspect that children, from whom cataracts had been extracted, had a notion of distance the first moment they were enabled to see. In the instance particularly of a young gentleman from Ireland, fourteen years old, from each of whose eyes I extracted a cataract, in the year 1794, in the presence of Dr. HAMILTON, Physician to the London Hospital, and who, before the operation, assured me, as did his friends, that he never had seen the figure of any object, Dr. HAMILTON and myself were much astonished by the facility with which, on the first experiment, he took hold of my hand at different distances, mentioning whether it was brought nearer to, or carried further from him, and conveying his hand to mine in a circular direction, that we might be the better satisfied of the accuracy with which he did it. In this case, however, and in others of a like nature, although the patients had certainly been blind from early infancy, I could not satisfy myself that they had not, before this period, enjoyed a sufficient degree of sight to impress the image of visible objects on their minds, and to give them ideas which could not afterwards be entirely obliterated. In the instance of Master W. however, no suspicion of this kind could occur; since, in addition to the declaration of himself and his mother, it was proved by the testimony of the surgeon who examined his eyes in the country, that the cataracts were fully formed before he was a year old. And I beg leave to add further, that on making inquiries of two children, between seven and eight years of age, now under my care, both of whom have been blind from birth, and on whom no operation has yet been performed, I find that the knowledge they have of colours, limited as it is, is sufficient to enable them to tell whether coloured objects be brought nearer to, or carried further from

them, for instance, whether they are at the distance of two inches or four inches from their eyes; nor have either of them the slightest suspicion, as is related of Mr. CHESELDEN's patient, that coloured objects, when held before them, touch their eyes.

But the judgment which Master W. formed of the different distances of objects, was not the only instance in which he differed from Mr. CHESELDEN's patient; who, we are informed, "did not know the figure of any thing, nor any one thing from "another, however different in shape and magnitude;" for Master W. knew and described a letter, not only as white, but also as square, because it had corners; and an oval silver box, not only as shining, but also as round, because it had not corners: he likewise knew, and called by its name, a white stone mug, on the first day he obtained his sight, distinguishing it from a bason, because it had a handle. These experiments were made in the presence of two respectable persons, as well as myself; and they were several times repeated, to convince us that we could not be mistaken in them. I mention the circumstance, however, with much diffidence, being aware that the observations not only differ from those that are related of Mr. CHESELDEN's patient, but appear, on the first statement, to oppose a *principle* in optics, which I believe is commonly and justly admitted, that the senses of sight and feeling have no other connection than that which is formed by experience; and, therefore, that the ideas derived from feeling can have no power to direct the judgment, with respect either to the distance or form of visible objects. It should be recollected, however, that persons who have cataracts in their eyes, are not, in strictness of speech, blind, though they are deprived of all useful sight. The instances I

have adduced prove, that the knowledge they have of colours is sufficient to give them some idea of distance, even in their darkest state. When, therefore, their sight is cleared by the removal of the opaque crystalline, which intercepted the light, and the colour of objects is thereby made to appear stronger, will it be difficult or unphilosophical, to conceive that their ideas of distance will be strengthened, and so far extended as to give them a knowledge, even of the outline and figure of those objects with the colour of which they were previously acquainted?

The case which I have here related appears to deserve notice, not only on account of the observations that were made by the patient on recovering his sight, but also on account of the hint which it affords to surgeons, relative both to the mode in which the cataract may best be removed, when children are born with this disorder, and the time when it is most proper to perform the operation.

The Baron DE WENZEL, in his ingenious Treatise on the Cataract, with great force of reasoning, deduced from the long and successful experience of his father and himself, recommends, in all cases of this disorder, without making any exceptions, the operation of extraction, in preference to that of depression; and I believe it is now generally acknowledged by medical men, that, in the more common cases, his decision, as to the mode of operating, is perfectly well founded. The Baron admits that the operation is not so certain a cure in children as it is in persons of a more advanced age; both on account of their untractableness, and because, in them, the opacity of the crystalline is not unfrequently accompanied with an opacity in the capsule that contains it. On these accounts, when children are born with this disorder, he advises to postpone the operation, until they

are old enough to be made sensible of the loss they sustain by the want of sight, and have firmness of mind to submit patiently to the means that are requisite in order to obtain it. Influenced by this opinion of the Baron, and believing the operation of extraction to be so much superior to that of depression, that the latter ought not, on any occasion, to have the preference, I have given advice, in the cases of a considerable number of children who were born with this disorder, to postpone every attempt to relieve them, until they were thirteen or fourteen years old. Prior to this time, it did not appear to me that children could be depended upon to submit, with due steadiness, to the repeated introduction of instruments, which is sometimes necessary in extracting the cataract; and, even at this age, the eyes of some are so small, and in such a constant rolling motion, that it is almost impossible properly to accomplish the operation. The Portuguese lad, whose case has been related, afforded an instance of this kind; and I consider it as a fortunate circumstance that it came under my notice, since, in some degree, it may be said to have obliged me to examine, more attentively than I had before done, the advantages and disadvantages of the operation of depression; which operation, being more easy to perform than that of extraction, has certainly this advantage in the cases of children, (to which alone I here advert,) that it may be performed with equal safety when they are only seven years of age, as it may at any subsequent period of their lives.

It is well known that the late Mr. POTT, who published his remarks on the cataract in the year 1775, was a strenuous advocate for this operation; and, though he appears to me to have much under-rated the advantages of extraction, it must be allowed that he makes many just and highly pertinent observa-

tions on the use of the couching needle, in those cases where the cataract is soft, or fluid. Mr. POTT considered this as a very common state of the disorder; and does not make any distinction between the cataract when it attacks grown persons, and when children are born with it. In the former case, experience inclines me to believe, that the cataract is very rarely fluid, or even soft; whereas, in the latter, I have always found it, agreeable to the observation of the Baron DE WENZEL, in one or other of these states. Although, therefore, in the case of grown persons, the operation of extraction appears to me to have very great advantages over that of depression, yet, in the case of children, I can readily accede to almost the whole that Mr. POTT advances in favour of depression. If the couching needle be passed in the way in which it is usually introduced to depress the cataract, and thereby a large aperture be made in the capsule of the crystalline, (which operation may be performed with perfect safety, and with very little pain to the patient, whilst the eye is fixed with a speculum oculi,) the opaque crystalline, being thus brought into contact with the aqueous and vitreous humours, will, in a shorter or longer space of time, according to its degree of softness, be absorbed; and, if there be not an opacity in the capsule, as well as in the crystalline, the pupil will become clear, and the patient will acquire a very useful sight. If, in addition to the opacity of the crystalline, the capsule be also opaque, and, in consequence of this, the operation do not prove successful, the eye will nevertheless be perfectly uninjured, and it will be as fit, at a subsequent period, to have the capsule extracted, as it would have been if no attempt of the above kind had been previously made.

From the foregoing observations, I flatter myself I shall be justified in deducing the following inferences.

First, When children are born blind, in consequence of having cataracts in their eyes, they are never so totally deprived of sight as not to be able to distinguish colours; and, though they cannot see the figure of an object, nor even its colour, unless it be placed within a very short distance, they nevertheless can tell whether, when within this distance, it be brought nearer to, or carried farther from them.

Secondly, In consequence of this power, whilst in a state of comparative blindness, children who have their cataracts removed, are enabled, immediately on the acquisition of sight, to form some judgment of the distance, and even of the outline, of those strongly defined objects with the colour of which they were previously acquainted.

Thirdly, When children have been born with cataracts, the crystalline humour has generally, if not always, been found either in a soft, or fluid state. If, therefore, it be not accompanied with an opacity, either in the anterior or posterior portion of the capsule, and this capsule be largely punctured with the couching needle, introduced in the way in which this instrument is usually employed to depress the cataract, there is reason to expect that the opaque matter will, sooner or later, be absorbed, the pupil become clear, and the sight be restored.

Fourthly, If, in addition to the opacity of the crystalline humour, its capsule be also opaque, either in its anterior or posterior portion, or in both, (which circumstance cannot be ascertained before the operation,) and, in consequence of this, the operation above mentioned should not prove successful, it will

not preclude the performance of extraction afterwards, if this be thought adviseable.

Fifthly, The operation above mentioned being much more easy to perform than that of extraction, and it being possible to fix the eye with perfect safety during its performance, by means of a speculum oculi, it may be undertaken at a much earlier age than the latter operation; and a chance may of course be given to the patient, of receiving instruction, without that loss of time which has usually been thought unavoidable, when children are born with this disorder.*

* It ought to be mentioned, that about a month after the above mentioned operation on Master W.'s left eye, I performed a similar operation on the right eye of the same young gentleman. Although he behaved with great firmness on the first occasion, it was not without considerable difficulty that his head was kept steady on the second. The operation, however, gave him very little pain, and no inflammation followed; but the opacity afterwards was not diminished; and he did not acquire any additional sight from this eye. There was an evident mark in that part of the capsule where the couching needle pierced it; though the aperture was too small to admit a sufficient number of rays of light to give an idea of objects. It seems probable that the want of success, in this instance, was owing to an opacity in the capsule, which was incapable of being absorbed. The eye, however, is as fit to have the aperture in the capsule enlarged, or a portion of it removed, when the patient is of a proper age, as if no operation had been previously performed.

I beg leave also to add, that since these pages were put together, a case has come under my care, which seems to afford a confirmation of the remarks that have been offered respecting the state of the cataract in children, and the effects that are likely to be produced by the operation of puncturing the capsule that contains it. A young lady, eighteen years old, was put under my care, who had been blind from an early part of her infancy. She had a cataract completely formed in both eyes; and in each there were three or four opaque spots, more white than the rest, which seemed to lie on the surface of the opaque crystalline. I punctured the capsule of each with a couching needle, according to the proposition in the preceding pages, in the presence of Mr. Scott, surgeon, in St. Alban's-street. The operation gave her no pain; and, in the course of a few days, the opacity was evidently diminished, particularly in the right eye, the patient discovering the colour of objects more plainly than before, but being still

unable to distinguish their figure. At the end of a month, finding no further improvement in her vision, it appeared to me most probable that the remaining opacity was situated in the capsules. I therefore determined to extract either a part or the whole of each of them. The incisions of the cornea were made in the usual manner; after which, I punctured the anterior parts of both the capsules with the sharp end of a gold curette. The punctures became immediately transparent, without affording an issue to the liquor Meibomii, or any other humour. From hence it seems evident, that nothing was contained within the capsules, or, in other words, that the crystalline humours were absorbed; and it appears to me highly probable, that their absorption had been occasioned by the previous operation of puncturing their capsules with the couching needle. I dilated the new punctures with the end of the curette; and afterwards, being still afraid that the apertures in the capsules might not be large enough to admit a sufficient number of the rays of light, I removed a portion of each of them with a small forceps. This was accomplished in the left eye, without occasioning the discharge of any part of the vitreous humour; and, in the right, the quantity of this humour that came away was very small. In the course of a week, the inflammation that followed the operation was nearly removed; a large portion of both pupils was quite clear; and the young lady distinguished objects with quickness and precision.

XX. *An Account of some* Galvanic *Combinations, formed by the Arrangement of single metallic Plates and Fluids, analogous to the new* Galvanic *Apparatus of Mr.* Volta. *By Mr.* Humphry Davy, *Lecturer on Chemistry in the Royal Institution. Communicated by* Benjamin Count of Rumford, *V.P.R.S.*

Read June 18, 1801.

I. All the Galvanic combinations analogous to the new apparatus of Mr. Volta, which have been heretofore described by experimentalists, consist (as far as my knowledge extends) of series containing at least two metallic substances, or one metal and charcoal, and a stratum of fluid. And it has been generally supposed, that their agencies are, in some measure, connected with the different powers of the metals to conduct electricity. But I have found that an accumulation of Galvanic influence, exactly similar to the accumulation in the common pile, may be produced by the arrangement of single metallic plates, or arcs, with different strata of fluids.

The train of reasoning which led to the discovery of this fact, was produced by the observation of some phænomena relating to the connection of chemical changes with the evolution of Galvanic power.

It appeared, in several experiments, that series of double metallic plates, incapable of acting as Galvanic combinations, when arranged in the proper order, with portions of water, were readily made to produce Galvanic effects, by being alternated with

acids, or other fluids capable of oxidating one only of the metals of the series. Thus, double plates, composed of silver and gold, (metals which have been supposed to differ very little in their powers of conducting electricity,) produced GALVANIC action, when placed in contact, in the common order, with cloths moistened in diluted nitric acid. And copper and silver acted powerfully with nitrate of mercury.

These facts induced me to suppose, that the alternation of two metallic bodies with fluids, was essential to the production of accumulated GALVANIC influence, only so far as it furnished two conducting surfaces of different degrees of oxidability; and that this production would take place, if single metallic plates could be connected together by different fluids, in such a manner that one of their surfaces only should undergo oxidation, the arrangement being regular.

On this supposition, I made a number of experiments on different arrangements of single metals and fluids; and, after many various processes, I was enabled to ascertain, that many of these arrangements could be made active, not only when oxidations, but likewise when other chemical changes were going on in some of their parts.

In describing the different GALVANIC combinations formed by single metallic plates and fluids, I shall divide them into three classes, following, in the arrangement, the order of time with regard to discovery.

II. The first and most feeble class is composed, whenever single metallic plates, or arcs, are arranged in such a manner that two of their surfaces, or ends opposite to each other, are in contact with different fluids, one capable, and the other incapable, of oxidating the metal. In this case, if the series are

numerous, and in regular alternation, GALVANIC influence will be accumulated, analogous, in all its effects, to the influence of the common pile.

Tin, zinc, and some other easily oxidable metals, act most powerfully in this class of combinations.

If pieces of polished tin, about an inch square and $\frac{1}{20}$ of an inch thick, be connected with woollen cloths of the same size, (moistened, some in water, and some in diluted nitrous acid,) in the following order, tin, acid, water, and so on, till twenty series are put together, a feeble GALVANIC battery will be formed, capable of acting weakly on the organs of sense, and of slowly producing the common appearances in water; the wire from the oxidating surface of the plates evolving hydrogen; and the wire from the non-oxidating surface (when of silver) depositing oxide.

In all cases, when the batteries of the first class are erected perpendicularly, the cloth moistened in acid must be placed under the cloth moistened in water; and, in this arrangement, as the acid is specifically heavier than water, little or no mixture of the fluids will take place.

When zinc is employed, on account of its rapid oxidation in water containing atmospheric air, three cloths should be used; the first moistened in weak solution of sulphuret of potash, (which is possessed of no power of action upon zinc, and which prevents it from acting upon the water;) the second moistened in a solution of sulphate of potash, of greater specific gravity than the solution of sulphuret; and the third wetted in an oxidating fluid specifically heavier than either of the solutions. In this case, if the order be as follows, zinc, oxidating solution, solution of sulphate of potash, solution of sulphuret of potash, very

little mixture of the fluids, or chemical action between them, will take place: and an alternation of twelve series of this kind, forms a battery capable of producing sensible effects.

III. The second class of GALVANIC combinations with single plates is formed, when plates, or arcs, composed of a metallic substance capable of acting upon sulphurated hydrogen, or upon sulphurets dissolved in water, are formed into series, with portions of a solution of sulphuret of potash, and water, in such a manner that one side of every plate, or arc, is in contact with water, whilst the opposite side is acted on by the solution of sulphuret. Under these circumstances, when the alternation is regular, and the number of series sufficiently great, GALVANIC power is evolved; and water, placed in the circuit with silver wires, is acted on; oxide being deposited on the wire connected with the side of the plate undergoing chemical alteration, whilst hydrogen is evolved from the side in contact with water.

Silver, copper, and lead, are each capable of forming this combination. Plates made from either of those metals, may be arranged with cloths, (moistened, some in water, and others in solution of sulphuret of potash,) in the following order, metal, cloth moistened in sulphuret of potash, cloth moistened in water, and so on.

Eight series will produce sensible effects; and the wire from the top of the pile produces oxide.

Copper is more active, in this class of batteries, than silver; and silver more active than lead.

IV. The third and most powerful class of GALVANIC batteries, constructed with fluids and single metals, is formed, when metallic substances oxidable in acids, and capable of acting on solutions of sulphurets, are connected, as plates, with oxidating

fluids and solutions of sulphuret of potash, in such a manner that the opposite sides of every plate may be undergoing different chemical changes; the mode of alternation being regular.

The same metals that act in the second class, may be used in the third class; and the order of their powers is similar. The pile may be erected in the same manner as the pile with zinc in the first class; the cloths moistened in acid being separated from those moistened in solution of sulphuret, by a third cloth, soaked in solution of sulphate of potash.

Three plates of copper, or silver, arranged in this manner, in the just order, produce sensible effects; and twelve or thirteen series are capable of giving weak shocks, and of rapidly producing gas and oxide in water; the wire connected with the oxidating end of the apparatus evolving hydrogen; and the wire attached to the end acting on the sulphuret, depositing oxide when composed of silver, and generating oxygen when of gold.

V. In all the single metallic piles constructed with cloths, the action is very transient: the decomposition of the acids, and of the sulphurets, is generally completed in a few minutes; and, in consequence, the GALVANIC influence ceases to be evolved. The arrangement of all the different series may, however, (by means of an apparatus constructed after the ideas of Count RUMFORD,) be made in such a manner as to give considerable permanency to their effects. This apparatus is a box, covered with cement incapable of conducting electricity, and composed of three pieces of mahogany, each containing grooves capable of receiving the edges of the different plates proper for composing the series. One half of these plates must be composed of horn, or glass, and the other half of metallic substances; and

the conductors of electricity, and the non-conductors, must be alternately cemented into the grooves, so as to form water-tight cells.

When the apparatus is used, these cells are filled, in the GALVANIC order, with different solutions, according to the class of the combination; and connected in pairs with each other, by slips of moistened cloth, carried over the non-conducting plates.

A combination of fifty copper-plates, arranged in this manner, with weak solutions of nitrous acid, or nitrate of ammoniac, and sulphuret of potash, gives pretty strong shocks, rapidly evolves gas from water, and affects the condensing electrometer.

It does not lose its power of action for many hours; and, when this power is lost, it may be restored by the addition of small quantities of concentrated solutions of the proper chemical agents to the fluids in the different cells.

From two experiments made on copper and silver, it would appear, that the single metallic batteries act equally well, when the metals made use of are slightly alloyed, and when they are in a state of purity.

XXI. *A Continuation of the Experiments and Observations on the Light which is spontaneously emitted from various Bodies;* with some Experiments and Observations on solar Light, when imbibed by* Canton's *Phosphorus. By* Nathaniel Hulme, *M. D. F. R. S. and A. S.*

Read June 18, 1801.

SECTION XI.

The Effects of various aerial Fluids on spontaneous Light.

INTRODUCTION.

THE apparatus employed for experiments with any kind of air, unless otherwise expressed, consisted of the following parts: 1. A tea-saucer, holding about three ounces of water. 2. A wide-mouthed phial, which would contain about ten ounces of liquid. 3. A small wooden stand, composed of a slender pillar or pin, nearly four inches high, fixed into a round base, a little more than an inch in diameter, and half an inch thick. This stand was fastened by strong thread to the middle of a piece of flat lead, such as lines Chinese tea-chests, having holes in it to admit the thread; the lead was about three inches square, and doubled, to give it weight and stability. The top of the pillar was made pointed; and a round piece of cork, about an inch in diameter and half an inch thick, was fixed upon it, by means of a superficial hole bored in its under part with a gimlet.

* See Phil. Trans. for 1800, page 161.

When the whole apparatus was put in use, the phial was filled with cold pump water, in a pneumatic tub, then inverted, and the species of air to be employed was let up into it, to the quantity of about eight ounces. The subject for experiment being applied to, or fastened upon, the top of the cork, the stand was placed on the tea-saucer, and then introduced, under water, into the phial containing the air. The whole apparatus, being now supported by the tea-saucer, with water in it, was deposited in the laboratory for experiments on light. By this contrivance, the experiments were made in about eight ounces of air, by measure, confined above two ounces of water.

§ 1.

The Effects of common or atmospherical Air on spontaneous Light.

EXPERIMENTS.

Exper. 1. Two fresh herrings were hung up together in the laboratory, so as to touch each other at their flat sides; and it was observed that the parts in contact remained dark, while those exposed to the open air became very luminous.

Exper. 2. Another fresh herring was laid upon a piece of thick brown paper, and placed in the laboratory. On examination, the next evening, the upper part, which was exposed to the air, was very lucid; but the underside, lying upon the paper, remained quite dark.

Exper. 3. A luminous herring was divided transversely quite through its middle fleshy part; but the inside was perfectly dark. On the following night, that which before was dark had become luminous.

Exper. 4. At 9 P. M. a piece of fresh herring, of about three drams in weight, was introduced above water, into about eight ounces of atmospherical air. On the second night it was become luminous; on the third and fourth, it continued shining; and on the fifth the light was extinguished. This experiment was frequently repeated, with both the flesh of herring and of mackerel, and nearly with the same result.

Exper. 5. The cork of the apparatus was well smeared with the luminous matter of a mackerel, and then introduced above water. It continued to shine finely all that evening; and the light was not quite extinct on the succeeding night.

Exper. 6. Another cork was illuminated with herring-light, at half an hour past six P. M. and introduced above water. It remained very bright at eleven; and retained a glimmering light the next evening. The two last experiments were often repeated, and, in general, with similar effects. It may not, however, be improper to observe, that the illumination of the cork did not always continue so long as twenty-four hours; for it must, of course, vary according to the quantity of luminous matter applied, and its degree of brilliancy.

Exper. 7. A large piece of rotten wood was received from the country, which shone only in one place. The luminous portion was sawed off for use, and the dark part left in the laboratory. On going into the laboratory, the second night after this operation, I was surprised to see the dark piece, which had been left there, very lucid in several places where small splinters had been broken off in sawing; many shining fragments also lay scattered on the floor.

Exper. 8. A quantity of rotten wood, moderately shining,

was blown upon for some time with a pair of bellows; but I could not perceive that this had any effect on the light, so as to render it more vivid.

Exper. 9. A small piece of shining wood was tied upon one of the corks of the apparatus, and introduced above water, where it continued lucid until the fifth night. In another experiment, the light was extinguished on the fourth night; and in a third much sooner.

Exper. 10. A living glow-worm, in a shining state, was submitted to the action of a pair of bellows; but the continuance of the blast did not apparently increase its glowing quality.

Exper. 11. A very luminous dead glow-worm was fixed upon a cork of the apparatus, by means of a small pin, and then put into the phial, above water. It continued to shine as vividly as it did when in the open air, forming a pure white light, of a circular shape.

OBSERVATIONS.

Obs. 1. These experiments prove, that objects which abound with spontaneous light in a latent state, such as the herring, mackerel, and the like, do not emit it when deprived of life, except from such parts as have been some time in contact with the air.

Obs. 2. They likewise show, that the blast of a pair of bellows does not increase this species of light, as it does that which proceeds from combustion.

§ II.

The Effects of oxygen Gas or vital Air on spontaneous Light.*

EXPERIMENTS.

Exper. 1. A piece of fresh herring, of about three drams weight, was introduced above water, into eight ounces of oxygen gas. On the second night it was observed to be faintly luminous; on the third, the quantity of light was increased; on the fourth, it continued nearly in the same state; and on the fifth the light was diminished.

Exper. 2. A piece of very fresh mackerel, of the same magnitude, was also put above water. On the subsequent evening it was pretty lucid, and continued the same on the night following.

Exper. 3. At 9 P. M. a cork, finely illuminated with mackerel-light, was introduced above water: it continued very lucid at eleven. On the next evening it was dark.

Exper. 4. Another cork, rendered luminous with the same kind of light, was put above water at 10 P. M. The next morning, at six o'clock, only a glimmer of light was perceived, and at 10 P. M. it was extinct.

Exper. 5. At 9 P. M. a fragment of shining wood was introduced above water: it was observed to be still very luminous at eleven; but the light was not quite so vivid, nor so extended in breadth, as when the wood was put in. On the succeeding night, at eight o'clock, it remained faintly lucid.

Exper. 6. A little after 8 P. M. another fragment of wood,

* The oxygen gas made use of was obtained from manganese, by means of heat.

shining very brightly, was introduced above water, into the same air that was used in the last experiment: it continued very luminous at eleven; but the light was diminished in quantity. On the next evening it was found to be extinguished.

Exper. 7. The same air was employed again at 8 P. M. with a pretty large and thick fragment of wood, uncommonly lucid: its light continued vivid and broad at half an hour past eleven. The following night, at eight o'clock, the light was still somewhat extensive and bright.

Exper. 8. In three other experiments with shining wood, in fresh oxygen gas, the light was totally extinguished in the space of twenty-four hours.

Experiments were made, at the same time, and in the same manner, with atmospherical air and shining wood; but it was not very evident that the wood shone more vividly in the latter air than it did in the oxygen gas.

Exper. 9. A living glow-worm was put into a two-ounce phial, with a glass stopple, containing pure oxygen gas, and kept therein for some time. It was then taken out, and exposed to the open air; but no difference, either in the brilliancy or the quantity of its light, could be discovered.

Exper. 10. A luminous dead glow-worm was then inclosed in about five ounces of the gas; but no increase of its shining quality could be perceived.

Exper. 11. At six o'clock P. M. a shining dead glow-worm was introduced above water into oxygen gas: it continued very lucid therein at 7 P. M. shewing a pure white light. It was then taken out, and put above water into atmospherical air, where it shone, to all appearance, as splendidly as it did when it was in the oxygen gas.

OBSERVATION.

It appears, from these experiments, that oxygen gas does not act upon this kind of light, so as to render it much more vivid than it is in atmospherical air; which is quite contrary to what some authors have alleged.

§ III.

The Effects of azotic Gas on spontaneous Light.

1. *Azotic Gas, obtained from lean muscular Flesh and diluted nitric Acid, in a very low Heat, as recommended by M. de* FOURCROY.

EXPERIMENTS.

Exper. 1. A piece of fresh mackerel, weighing about three drams, was introduced above water, into about eight ounces of this azotic gas; and it was retained therein five days, without emitting any light.

Exper. 2. About the same quantity of fresh herring was then put above water, into the same gas used for the last experiment, and remained in it for the space of three days, in a dark state. This experiment was repeated, and with a similar result.

Exper. 3. At 45 minutes past 7 P. M. a cork, finely illuminated with mackerel-light, was put above water into the gas, and it was found pretty luminous at eleven. On the next evening, at eight o'clock, it still exhibited a faint degree of light.

A similar experiment was made, at the same time, in atmospherical air. At 11 P. M. the cork was but moderately luminous; and on the next evening it was dark.

Exper. 4. At 40 minutes past 7 P. M. another cork, rendered very luminous with herring-light, was introduced above water. This cork, at 11 P. M. was not found so lucid as that in the third experiment. On the next evening, a glimmer of light was still perceptible.

Exper. 5. A fragment of very shining wood was introduced above water, into this gas; and it was rendered dark in about 15 minutes.

Exper. 6. The experiment was repeated; and the light was again extinguished in about 15 minutes. In another experiment, it was extinguished in about 25 minutes.

II. *Atmospheric Air rendered azotic, by burning Spirit of Wine in it, when confined above Water.*

Exper. 7. A portion of fresh herring, of about three drams, was put above water, into this azotic gas, at 5 P. M. On the second evening, a spark of light was observable; on the third, the quantity of light was increased; on the fourth, it was again diminished.

Exper. 8. At 3 P. M. the usual quantity of herring was introduced above water. On the second night, it remained dark; on the third, it was moderately luminous; on the fourth, it was less so; on the fifth, the light was extinct.

Exper. 9. A piece of fresh mackerel was next put above water, at 11 A. M. On the second evening, it was found to be slightly luminous; it remained so on the third; on the fourth, it was dark.

Exper. 10. Another piece of fresh mackerel was introduced above water, at 3 P. M. On the second night, it was found to be slightly luminous; but on the third, it was dark; and no

more light was emitted, though it was kept in the gas for the space of four days.

Exper. 11. A cork, made very luminous with herring-light, was put above water, into this gas, at 20 minutes past 8 P. M. and it continued very lucid at eleven. The next evening, at ten o'clock, the light was nearly extinguished.

A similar experiment was made, at the same time, in common atmospherical air, and with the same result.

Exper. 12. Another cork was introduced above water, with herring-light, at 40 minutes past 7 P. M. and it remained pretty luminous at eleven. On the following night, it was nearly extinct.

III. *The last mentioned azotic Gas, after being washed with Lime Water.*

Exper. 13. A piece of herring, of about three drams weight, was put above water, into this azotic gas, at 5 P. M. On the second night, it was dark; on the third, very lucid; and on the fourth, the same.

Exper. 14. The experiment was repeated, on a piece of herring, at 3 P. M. On the second evening, it was dark; on the third, pretty luminous; on the fourth, it was less so; and on the fifth, only a faint light remained.

Exper. 15. A portion of fresh mackerel was then put above water, at 11 A. M. On the second night, it was observed to be moderately shining; on the third, the light was extinct.

Exper. 16. Another piece of fresh mackerel was introduced above water, at 3 P. M. On the second evening, it was slightly luminous; on the third, it was dark, and continued so during the four succeeding nights.

Exper. 17. A cork, finely illuminated with herring-light, was next introduced above water, into this gas, at 20 minutes past 8 P. M. The light was much diminished at 45 minutes past 8; at 11 the cork had become almost dark. On the following night, a glimmer was still apparent.

Exper. 18. Another cork, made very luminous with herring-light, was put above water, at 40 minutes past 7 P. M. and it continued pretty lucid at eleven. On the next evening, the light was merely visible.

A similar experiment was made, at the same time, in atmospheric air, and with nearly the same effect.

OBSERVATION.

It is a remarkable circumstance, that azotic gas, which is incapable of supporting light from combustion, should be so favourable to the spontaneous light which is emitted from fishes, as to preserve its existence and brilliancy for some time, *when applied upon a cork;* yet that it should prevent the *flesh* of the herring and the mackerel from becoming luminous, and also extinguish the light proceeding from rotten wood.

§ IV.

The Effects of hydrogen Gas or inflammable Air on spontaneous Light.*

EXPERIMENTS.

Exper. 1. At 9 P. M. a piece of fresh herring, weighing about three drams, was introduced above water, into hydrogen gas.

* This gas was obtained from zinc and diluted sulphuric acid.

It was retained therein three days and three nights, without emitting any light. It was then taken out, and exposed to the action of atmospherical air. On the following night it was found to be luminous; but was dark again on the next night.

Exper. 2. Another piece of fresh herring was put above water, at 6 P. M. This was also kept in the gas the same length of time, without producing any light. It was then exposed to the open air, and inspected two successive nights, but it remained dark.

Exper. 3. The same experiment was then made with a piece of mackerel, which was taken out on the fourth night, without producing any shining appearance. The next evening, it emitted a very faint light, which did not continue twenty-four hours.

Exper. 4. A cork, brilliantly illuminated with mackerel-light, was introduced above water; and the light was extinguished in about the space of an hour.

Exper. 5. At 39 minutes past 9 P. M. another luminous cork was put above water; it lost some of its light pretty soon, but was not extinct at twelve.

Exper. 6. A cork, with herring-light, was introduced above water, at 23 minutes past 6 P. M. The light gradually diminished, and was only faintly visible at eleven.

Exper. 7. A fragment of very shining wood was put above water, at 9 P. M. and was dark at eleven.

Exper. 8. Another fragment was put above water, at 40 minutes past 8 P. M. at 50 the light was much diminished, and at 8 minutes past 9 the shining ceased. The wood was then taken out, and exposed to the open air, when the light revived in a very beautiful manner.

Exper. 9. A piece of uncommonly shining wood was introduced above water, at 58 minutes past 8 P. M. it remained for a short time very luminous, but at 25 minutes past 9 the light was greatly diminished; at 20 past 10 it was nearly extinguished; and at 29 past 10 was quite dark. It was then exposed to atmospheric air, and the light revived very brightly.

Exper. 10. The same experiment was repeated, at 35 minutes past 8 P. M. the shining property was much diminished at 9; and at 10 it was very faint. The next evening, it continued merely visible. The wood was now taken out, and the light soon revived very strongly. The following night, it was still moderately lucid; but on the next evening nearly extinct.

Exper. 11. Finding, by the above experiments, that the light of shining wood was extinguished by this species of gas, and restored by atmospheric air, the following three trials were made, to discover, in some degree, how long its light might be kept in a latent state, and then be revived. At 9 P. M. several fragments of shining wood, tied up in a piece of gauze, were introduced above water, into the hydrogen gas, and the light was gradually extinguished during that evening. They were kept there in that dark state 48 hours, were then taken out, and exposed to the open air, when, after a little time, the light re-appeared.

Exper. 12. On the 2d of October, another fragment of exceedingly shining wood, two inches and an half long, and pretty thick, was put above water in the evening, and its light was gradually extinguished. On the second night, it was taken out perfectly dark, but its light recovered by degrees, and became brilliant. It was introduced again, that evening, into the same gas, and its light disappeared. On the third night, it was again

exposed to the open air, and the light revived as before. It was then reinstated and extinguished, and continued in a dark state, from the third to the fifth night, when, being again taken out, it soon shone in a pretty vivid manner. It was again introduced and extinguished as usual; and no observation was made of it, from some accidental circumstance or other, until the 10th of November in the evening, when it was taken out, and exposed to the open air for a length of time, but the light did not revive.

Exper. 13. A third fragment, somewhat larger than the former, and equally luminous, was put above water, at the same time as the one in the last experiment, where it was soon deprived of its light. It was retained there, in a dark state, from the 2d of October till the 10th of November; it was then taken out, and exposed to the action of atmospherical air, for several days, but there was no return of light.

Exper. 14. About 7 P. M. a shining dead glow-worm was introduced above water into the gas, and its light was soon extinct. It was then exposed to the open air, where, in a very short time, it shone as brightly as before.

Exper. 15. At half an hour past 9 P. M. the same glow-worm was again introduced above water; when its light in a short time disappeared. It was taken out for exposure to common air at 11, and its glowing property was immediately restored. It was again replaced in the gas, where it soon lost all its light a second time, and was kept in that dark state for 24 hours; when taken out, it continued dark for a little time, and then the insect gradually recovered its pristine splendour.

OBSERVATION.

From these experiments we learn, that hydrogen gas, in general, prevents the emission of spontaneous light, and also extinguishes it when emitted; but, at the same time, it does not hinder its quick revival, when the subject of the experiment is again exposed to the action of atmospherical air; although the light may have been a considerable time in an extinguished state.

§ V.

The Effects of carbonic Acid Gas or fixed Air on spontaneous Light.*

EXPERIMENTS.

Exper. 1. At 10 P. M. a piece of fresh herring, weighing about three drams, was suspended in a wide-mouthed ten-ounce phial, filled with carbonic acid gas, and closed with a cork and bladder. It was retained there for three successive nights; but emitted no light.

Exper. 2. The same experiment was made with a piece of herring, which was beginning to be luminous. On the next evening, the illumination was found to be extinct: nevertheless the herring was still kept in the gas, for three nights longer, but did not become lucid.

Exper. 3. At 7 P. M. a piece of fresh mackerel was introduced above water, into a wide-mouthed bottle, holding 24 ounces, which was completely filled with carbonic acid gas,

* This gas was obtained from powdered chalk, or marble, and diluted sulphuric acid.

and supported by a tea-saucer that held about three ounces of water. On the second night it was dark, and continued the same on the third. It was then exposed to the influence of atmospherical air, and, on the next evening, it was pretty luminous, and likewise on the succeeding night.

Exper. 4. At 9 P. M. a cork, smeared with the luminous matter of a mackerel, was put into a five-ounce wide-mouthed phial, filled with carbonic acid gas, and then closed with a glass stopple. It continued to shine pretty vividly for some little time; then the light gradually diminished, so that at twelve, only a small spark remained.

Exper. 5. At 10 P. M. another cork, illuminated with mackerel-light, was introduced above water, into 24 ounces of the gas; and its light was nearly extinct at twelve.

Exper. 6. At 8 P. M. a fragment of shining wood was put above water, into 24 ounces of the gas; and it had not been long there before the light disappeared. It was then taken out, and exposed to the action of atmospheric air, when its shining property soon returned.

Exper. 7. Another fragment of brightly shining wood was introduced above water, into the same quantity of the gas, at 10 P. M. and the light was extinguished in the space of an hour. After this, it was exposed to the open air, and the light gradually revived.

Exper. 8. At 8 P. M. a luminous dead glow-worm was put above water into the gas; its glowing appearance gradually faded, and in a short time became quite invisible. It was then taken out, and the light, by degrees, re-appeared as vivid as before.

OBSERVATION.

This gas, we find, has also an extinguishing property, with respect to spontaneous light; but, in general, the light returns, if the object of experiment be taken out, and exposed to the open air.

§ VI.

The Effects of sulphurated hydrogen Gas on spontaneous Light.*

EXPERIMENTS.

Exper. 1. At noon, a piece of a very fresh mackerel, with a bright eye, was introduced above water, into 24 ounces of this gas, and was retained therein for three successive evenings, without emitting any light. It was then exposed to atmospheric air; yet it continued dark on the two following nights: but, on the third, it was very luminous, and remained so on the fourth and fifth.

Exper. 2. The same experiment was then made with a piece of fresh herring, which was also kept in the above gas, for about three nights, without being luminous. After exposure to common air, it did not emit any light during the first 24 hours. However, on the subsequent night, it began to shine, had a very bright light on the following evening, and continued shining for several succeeding nights.

Exper. 3. A cork, smeared with the luminous matter of a herring, was put above water, into 24 ounces of the gas; and

* This gas was obtained from sulphuret of potash and diluted muriatic acid.

the light was extinguished in less than an hour. The experiment was repeated in the same gas, and with the same result.

Exper. 4. A cork, illuminated with mackerel-light, was introduced into the same quantity of gas; and was dark in half an hour.

Exper. 5. A fragment of shining wood, being put into the gas, became dark in eight minutes. A second piece became dark in five minutes. They were then taken out, and continued dark all that evening. On the next evening, one of the pieces was uncommonly lucid.

Exper. 6. At 10 P. M. another fragment of brightly shining wood was introduced above water, into 24 ounces of the gas, and was extinct at eleven. It was then exposed to the open air; but there was no return of light that evening. On the following night, it was found pretty luminous.

Exper. 7. A finely shining dead glow-worm was next put above water, into this gas, and its light was quickly extinguished. In a second experiment, in the same gas, the light was much slower in its extinction. In both instances, after the insect was withdrawn, and placed in atmospheric air, the light gradually revived.

OBSERVATION.

It is apparent, by these experiments, that sulphurated hydrogen gas extinguishes spontaneous light much sooner than carbonic acid gas, and that, in general, the light returns much more slowly, when the subject is exposed to atmospheric air.

§ VII.

The Effects of nitrous Gas on spontaneous Light.*

EXPERIMENTS.

Exper. 1. A piece of fresh herring was introduced above water, into this gas, at 3 P. M. and remained there four nights, without emitting any light: it was then withdrawn, and exposed to common air, for the space of three nights; but did not become lucid.

Exper. 2. The same experiment was made with a piece of herring beginning to be luminous; but its light was gradually extinguished: it was detained in the gas for three nights, and taken out dark. It was then exposed to the open air, for the three subsequent nights; but its shining appearance did not return.

Exper. 3. A cork with luminous matter, introduced above water, into this species of gas, had its light, in general, extinguished in from 10 to 30 minutes; and, when taken into common air, its light very seldom re-appeared.

Exper. 4. Fragments of shining wood, above water, in nitrous gas, were likewise commonly rendered dark in a very short space of time, as in three or four minutes; sometimes a fragment, if uncommonly luminous, would not be extinguished in less than six or eight minutes; and very seldom would the light revive, on exposing the wood to atmospherical air.

Exper. 5. A dead shining glow-worm being put above water, into this gas, its light was quickly extinguished; but, after the insect was taken into the common atmosphere, the

* This gas was obtained from copper and diluted nitrous acid.

light gradually returned. The experiment was thrice repeated, and with the same result.

OBSERVATION.

This species of gas, we observe to have totally prevented the emission of light, and to have quickly extinguished that which had been emitted: likewise that the luminous objects which had been under its influence, (except the glow-worm) did not experience a revival of their light, when taken out, and kept for some time in common air.

§ VIII.

The Effects of a Vacuum on spontaneous Light.

EXPERIMENTS.

Exper. 1. A piece of shining wood, of a moderate size, was put under the receiver of an air-pump, in a dark room; in proportion as the air was extracted, the light was gradually extinguished, and at last reduced to a mere point, just visible, owing most probably to a small residuum of air, which is always left, even in the most perfect machine. Fresh air was then leisurely admitted, and the light was immediately revived in a very beautiful manner. This experiment was frequently repeated, and always with the like effect.

Exper. 2. Some luminous matter of a herring, uncommonly bright, was smeared upon a piece of red blotting paper, and then submitted to the operation of the air-pump. The light became fainter and fainter, as the inclosed air was withdrawn, and at last nearly vanished; but brightened up as before, on the

influx of fresh air. The experiment was repeated, and with the same result.

SECTION XII.

Experiments and Observations on solar Light, when imbibed by CANTON's *Phosphorus.*

§ I.

The Effects of Heat on imbibed solar Light.

I. *The imbibed Light is rendered more vivid by a* MODERATE *Degree of Heat.*

EXPERIMENTS.

Exper. 1. Having prepared some CANTON's phosphorus, and exposed it to the light of the sun, it was carried into the dark laboratory, to separate the illuminated parts from those that remained dark. In doing which, some luminous fragments were placed upon the palm of the hand, and retained there for some time, when it was observed, that the warmth of the hand considerably increased the degree of light.

Exper. 2. Some fragments of this illuminated phosphorus were put into a small phial, which was then closed with a cork, and suspended, by a string, in a quart of water heated to about 126°; by these means, the light was rendered much more vivid than before.

Exper. 3. Some other pieces of the illuminated phosphorus were dropped separately into a glass tube 32 inches long, and $\frac{7}{10}$ bore, filled with water at about 120°. The light of each piece became exceedingly bright, as soon as it entered the hot

water; and they all descended, very luminous, from the top to the bottom, some quickly and others slowly, according to their gravity, making a very pleasing experiment.

Exper. 4. A large wooden bowl, about 12 inches wide, was next filled with water heated to about 110°, and then a quantity of illuminated phosphorus, partly in the form of powder, and partly in pieces of different magnitudes, was scattered over the whole surface of the water; all which pieces fell, with increased splendour, to the bottom, where they preserved their light for some time.

II. *The imbibed Light is extinguished by a* GREAT *Degree of Heat.*

Exper. 5. Some fragments of the phosphorus, rendered luminous, were exposed to a greater degree of heat, namely, by casting them into a tin vessel containing two pints of boiling water. They flashed with increased light, as soon as they came in contact with the water, fell precipitately to the bottom, in a lucid state, and then were gradually extinguished.

Exper. 6. *In which the degree of heat was still increased.* A small bar of iron, of about an inch square, was made red-hot, and laid horizontally in the laboratory, until, by cooling, it nearly ceased to shine. Some pieces of illuminated phosphorus were then put upon it in succession, and the light, in a moment, glowed with uncommon lustre, but was quickly after totally extinguished.*

* Solar light, when received merely on a piece of white paper, may also be rendered more luminous by heat, and then extinguished by it, as appears from an experiment made by the late Mr. B. WILSON, whose book on phosphori I had not seen before this Paper was drawn up.

III. *The imbibed Light, after being in a latent State, is excited and rendered luminous by the Agency of Heat.*

Exper. 7. Some small pieces of the phosphorus, after having been illuminated, were deposited in the laboratory; when the light by degrees faded away, and became totally invisible. They were kept in this dark state for the space of ten days, and then placed one after another upon a heated bar of iron, as in the last experiment, upon which they quickly became exceedingly luminous.

From an experiment made by the ingenious Mr. CANTON, I observe, that some of his phosphorus, contained in glass balls hermetically sealed, and heated in the above manner, gave a considerable degree of light, after it had been kept in a state of darkness more than six months. Phil. Trans. Vol. LVIII. page 342.

§ II.

The Effects of Cold on imbibed Light.

EXPERIMENT.

About 15 grains of the phosphorus were put into a half-ounce phial, containing two drams of cold pump water, that had been deprived of its air by boiling. The phial was then corked, and exposed for some time to solar light, whereby the phosphorus became finely illuminated. In this state, it was immediately put into a frigorific mixture, composed of snow and sea-salt, and retained there about 30 or 40 minutes, when it was taken out, and the light found to be totally extinguished. The phial was then placed in some water, at about 60° temperature, and the

light gradually revived, and became as brilliant as before it had been exposed to the cold. This experiment was frequently repeated, and always with the same result.

I cannot but remark, that in the course of experiments on this subject, the superior power of solar over that of spontaneous light was very apparent. For, the first trials being made in small phials, containing only atmospheric air with the phosphorus, the light was with some difficulty totally extinguished; and, after the phials were taken out of the frigorific mixture, the temperature of the laboratory would commonly soon revive the light, which rendered the experiments not altogether satisfactory. Finding it thus somewhat difficult to extinguish solar light *in air*, recourse was had *to water*, in the manner above described. This answered perfectly well; for the water, when frozen, gave a substantial body, as it were, to the imbibed light of the phosphorus, so as to enable it to retain the excess of cold arising from the frigorific mixture; thereby making the experiments quite satisfactory. When the phosphorus was thus surrounded by ice, only a few minutes stay in the frigorific mixture would generally be sufficient for a total extinction.

OBSERVATION.

From these experiments, compared with those recited in my former Paper on spontaneous light, it appears that solar light, when imbibed by CANTON's phosphorus, is subject to the same laws, with respect to heat and cold, as the spontaneous light of fishes, rotten wood, and glow-worms.

P. S. In these experiments with solar light, the phosphorus was sometimes exposed to the direct rays of the sun, at other times to common day-light, in a northern aspect; and it was remarked, that it became somewhat more luminous by mere day-light, than by the rays of the sun.

It may also be proper to observe, that the above experiments were made with an improved preparation of CANTON's phosphorus. This improvement, which was first made by Dr. HIGGINS, consists in omitting the pulverization of the shells. His method was, after calcining the oyster-shells, to put the pieces, both great and small, in layers, into a crucible furnished with a cover, and to sprinkle flowers of sulphur between each layer. After they had remained some time in the furnace, they were taken out, suffered to cool, and then kept in a large bottle with a glass stopple. For this communication, I am indebted to Mr. LEWIS of Holborn, near Southampton-street, who has an extraordinary dark room, where, at times, he amuses his friends with some beautiful appearances, arising from solar light imbibed by phosphorus prepared as above directed. A still further improvement of this phosphorus, it appears to me, may be made by substituting precipitated sulphur for the flowers of sulphur; and the experiments of this section were chiefly made with phosphorus so prepared.

XXII. *Experiments on the chemical Production and Agency of Electricity. By* William Hyde Wollaston, *M. D. F. R. S.*

Read June 25, 1801.

Notwithstanding the power of Mr. Volta's electric pile is now known to be proportional to the disposition of one of the metals to be oxidated by the fluid interposed, a doubt has been entertained by many persons, whether this power arises from the chemical action of the fluid on the metal, or, on the contrary, whether the oxidation itself may not be occasioned by electricity, set in motion by the contact of metals that have different conducting powers.

That the oxidation of the metal is the primary cause of the electric phænomena observed, is, I think, to be inferred from the following experiments, which exhibit the Galvanic process reduced to its most simple state.

Exper. 1. If a piece of zinc and a piece of silver have each one extremity immersed in the same vessel, containing sulphuric or muriatic acid diluted with a large quantity of water, the zinc is dissolved, and yields hydrogen gas, by decomposition of the water: the silver, not being acted upon, has no power of decomposing water; but, whenever the zinc and silver are made to touch, or any metallic communication is made between them, hydrogen gas is also formed at the surface of the silver.

Any other metal beside zinc, which by assistance of the acid employed is capable of decomposing water, will succeed

equally, if the other wire consists of a metal on which the acid has no effect.

Exper. 2. If zinc, iron, or copper, are employed with gold, in dilute nitric acid, nitrous gas is formed, in the same manner, and under the same circumstances, as the hydrogen gas in the former experiment.

Exper. 3. Experiments analogous to the former, and equally simple, may also be made with many metallic solutions. If, for instance, the solution contains copper, it will be precipitated by a piece of iron, and appear on its surface. Upon silver merely immersed in the same solution, no such effect is produced; but, as soon as the two metals are brought into contact, the silver receives a coating of copper.

In the explanation of these experiments, it is necessary to advert to a point established by means of the electric pile.

We know that when water is placed in a circuit of conductors of electricity, between the two extremities of a pile, if the power is sufficient to oxidate one of the wires of communication, the wire connected with the opposite extremity affords hydrogen gas.

Since the extrication of hydrogen, in this instance, is seen to depend on electricity, it is probable that, in other instances, electricity may be also requisite for its conversion into gas. It would appear, therefore, that in the solution of a metal, electricity is evolved during the action of the acid upon it; and that the formation of hydrogen gas, even in that case, depends on a transition of electricity between the fluid and the metal.

We see, moreover, in the first experiment, that the zinc, without contact of any other metal, has the power of decom-

posing water; and we can have no reason to suppose that the contact of the silver produces any new power, but that it serves merely as a conductor of electricity, and thereby occasions the formation of hydrogen gas.

In the 3d experiment also, the iron by itself has the power of precipitating copper, by means, I presume, of electricity evolved during its solution; and here likewise the silver, by conducting that electricity, acquires the power of precipitating the copper in its metallic state.

The explanation here given receives additional confirmation from comparative experiments which I have made with common electricity; for it will be seen, that the same transfer of chemical power, and the same apparent reversion of the usual order of chemical affinities in the precipitation of copper by silver, may be effected by a common electrical machine.

The machine with which the following experiments were conducted, consists of a cylinder 7 inches in diameter, with a conductor on each side, 16 inches long, and $3\frac{1}{2}$ inches diameter, each furnished with a sliding electrometer, to regulate the strength of the spark received from them.

Exper. 4. Having a wire of fine silver, $\frac{1}{120}$ of an inch in diameter, I coated the middle of it, for 2 or 3 inches, with sealing-wax, and, by cutting through in the middle of the wax, exposed a section of the wire. The two coated extremities of the wire, thus divided, were immersed in a solution of sulphate of copper, placed in an electric circuit between the two conductors; and sparks, taken at $\frac{1}{10}$ of an inch distance, were passed by means of them through the solution. After 100 turns of the machine, the wire which communicated with (what is called) the negative conductor, had a precipitate formed on its surface, which,

upon being burnished, was evidently copper; but the opposite wire had no such coating.

Upon reversing the direction of the current of electricity, the order of the phenomena was of course reversed; the copper being shortly redissolved by assistance of the oxidating power of positive electricity, and a similar precipitate formed on the opposite wire.

Exper. 5. A similar experiment made with gold wires $\frac{1}{100}$ of an inch diameter, in a solution of corrosive sublimate, had the same success.

The chemical agency, therefore, of common electricity, is thus proved to be the same with the power excited by chemical means; but, since a difference has been observed in the comparative facility with which the pile of VOLTA decomposes water, and produces other effects of oxidation and de-oxidation of bodies exposed to its action, I have been at some pains to remove this difficulty, and can at least produce a very close imitation of the GALVANIC phenomena, by common electricity.

It has been thought necessary to employ powerful machines, and large Leyden jars, for the decomposition of water; but, when I considered that the decomposition must depend on duly proportioning the strength of the charge of electricity to the quantity of water, and that the quantity exposed to its action at the surface of communication depends on the extent of that surface, I hoped that, by reducing the surface of communication, the decomposition of water might be effected by smaller machines, and with less powerful excitation, than have hitherto been used for that purpose; and, in this hope, I have not been disappointed.

Exper. 6. Having procured a small wire of fine gold, and

given it as fine a point as I could, I inserted it into a capillary glass tube; and, after heating the tube, so as to make it adhere to the point and cover it in every part, I gradually ground it down, till, with a pocket lens, I could discern that the point of the gold was exposed.

The success of this method exceeding my expectations, I coated several wires in the same manner, and found, that when sparks from the conductors before mentioned were made to pass through water, by means of a point so guarded, a spark passing to the distance of $\frac{1}{8}$ of an inch would decompose water, when the point exposed did not exceed $\frac{1}{700}$ of an inch in diameter. With another point, which I estimated at $\frac{1}{1500}$, a succession of sparks $\frac{1}{20}$ of an inch in length, afforded a current of small bubbles of air.

I have since found, that the same apparatus will decompose water, with a wire $\frac{1}{40}$ of an inch diameter, coated in the manner before described, if the spark from the prime conductor passes to the distance of $\frac{4}{10}$ of an inch of air.

Exper. 7. In order to try how far the strength of the electric spark might be reduced by proportional diminution of the extremity of the wire, I passed a solution of gold in *aqua regia* through a capillary tube, and, by heating the tube, expelled the acid. There remained a thin film of gold, lining the inner surface of the tube, which, by melting the tube, was converted into a very fine thread of gold, through the substance of the glass.

When the extremity of this thread was made the medium of communication through water, I found that the mere current of electricity would occasion a stream of very small bubbles to rise from the extremity of the gold, although the wire, by which it communicated with the positive or negative conductor, was

placed in absolute contact with them. Hence it appears, that decomposition of water may take place by common electricity, as well as by the electric pile, although no discernible sparks are produced.

The appearance of two currents of air may also be imitated, by occasioning the electricity to pass by fine points of communication on both sides of the water; but, in fact, the resemblance is not complete; for, in every way in which I have tried it, I observed that each wire gave both oxygen and hydrogen gas, instead of their being formed separately, as by the electric pile.

I am inclined to attribute the difference in this respect, to the greater intensity with which it is necessary to employ common electricity; for, that positive and negative electricity, so excited, have each the same chemical power as they are observed to have in the electric pile, may be ascertained by other means.

In the precipitation of copper by silver, an instance of deoxidation (or phlogistication) by negative electricity has been mentioned: the oxidating power of positive electricity may be also proved, by its effect on vegetable blue colours.

Exper. 8. Having coloured a card with a strong infusion of litmus, I passed a current of electric sparks along it, by means of two fine gold points, touching it at the distance of an inch from each other. The effect, as in other cases, depending on the smallness of the quantity of water, was most discernible when the card was nearly dry. In this state, a very few turns of the machine were sufficient to occasion a redness at the positive wire, very manifest to the naked eye. The negative wire, being afterwards placed on the same spot, soon restored it to its original blue colour.

By Mr. Volta's apparatus, the same effects are produced in much less time.

Beside the similarity which has thus been traced between the effects of electricity excited by the common machine and those observed from the electric pile, I think it appears also probable, that they originate from the same source.

With regard to the latter, its power is now known to depend on oxidation; so also does the excitement in the former appear very much to depend on the same process; for,

Exper. 9. I have found, that by using an amalgam of silver or of platina, which are not liable to be oxidated, I could obtain no electricity. An amalgam of tin, on the contrary, affords a good degree of excitement. Zinc acts still better; but the best amalgam is made with both tin and zinc, a mixture which is more easily oxidated than either metal separately.

Exper. 10. But, as a farther trial whether oxidation assists in the production of electricity, I mounted a small cylinder, with its cushion and conductor, in a vessel so contrived that I could at pleasure change the contained air.

After trying the degree of excitement in common air, I substituted carbonic gas, and found that the excitement was immediately destroyed, but that it returned upon re-admission of atmospheric air.

In conformity to this hypothesis, we find that the metal oxidated is, in each case, in a similar state of electricity; for the cushion of the machine, by oxidation of the amalgam adhering to it, becomes negative; and, in the same manner, zinc oxidated by the accumulated power of an electric pile, or simply by action of an acid, is also negative.

This similarity in the means by which both electricity and GALVANISM appear to be excited, in addition to the resemblance that has been traced between their effects, shews that they are both essentially the same, and confirms an opinion that has already been advanced by others, that all the differences discoverable in the effects of the latter, may be owing to its being less intense, but produced in much larger quantity.

XXIII. *Farther Observations on the Effects which take Place from the Destruction of the Membrana Tympani of the Ear; with an Account of an Operation for the Removal of a particular Species of Deafness. By Mr.* Astley Cooper. *Communicated by* Everard Home, *Esq. F. R. S.*

Read June 25, 1801.

In the Paper which I had last year the honour of presenting to the Royal Society, I endeavoured to point out the effects which are produced upon the organ of hearing, by a partial loss, or entire destruction, of the membrana tympani.

From the facts therein detailed it appears, that an aperture in the membrana tympani does not diminish the power of the ear, and that even a complete destruction of the membrane is not followed by a total deprivation of the sense of hearing; a supposition which medical men have adopted, and common opinion has generally sanctioned.

Convinced of the importance of the subject, and desirous, as far as my other avocations would allow, of pursuing my inquiries, I have, since the publication of that Paper, examined more than twenty cases of a similar defect in the membrana tympani; and these instances have uniformly tended to confirm me in my former opinion, as to the use of the membrane, and the effects which follow from its loss.

Injury may arise to the membrana tympani, or its destruction take place, from various causes, of which the most common is,

a suppuration in the meatus auditorius. In persons of a delicate constitution and irritable habit, the wax secreted in the ear is liable to be hardened; this, by filling the meatus auditorius, gradually occasions deafness, and then excites inflammation and suppuration. In this case, if no mode of relief be resorted to, not only will the membrane lining the meatus, but also the membrana tympani itself be destroyed, the small bones of the tympanum discharged, and sometimes considerable exfoliations produced.

The membrana tympani is also not unfrequently injured by means of external violence. In Plate XXXIII. Fig. 4, a view is given of the membrane lacerated in different directions, by a blow upon the side of the head; an effect which probably was occasioned by the air in the meatus having been driven with violence upon the membrane.

The membrana tympani is also sometimes broken by attempts to remove extraneous bodies, which have been thrust into the meatus auditorius. Children, in their thoughtless pranks, often introduce small stones, pieces of slate-pencil, and even pins into their ears; in extracting which, I have known considerable lacerations made in the membrana tympani. Fig. 5, shews the membrane broken perpendicularly, in an attempt to remove a pin, which had been accidentally dropped into the meatus.

The membrana tympani may be easily seen in some persons, by directing the rays of the sun, or a condensed light from a common lamp, into the ear; but this is not the case in all; for the meatus differs considerably in different persons, both in its depth and diameter.

If the ear is clear from wax, the membrane has a bright tendinous appearance; and an aperture in it appears as a dark

spot, which, by the silvery surface of the membrane surrounding it, is rendered distinctly perceptible. If there be an aperture, air also, upon blowing the nose with violence, will be forced with a whistling noise through the ear. The smoke of tobacco may be driven from the mouth through the ear; or water may be injected from the ear into the throat.*

The effect produced upon the sense of hearing, by this defective state of the membrana tympani, varies according to circumstances. If there be a small aperture only, leaving the malleus with its natural attachment, no difference in the power of the organ is perceptible; the membrane vibrates, and communicates its vibrations, as before. If the whole of the membrane be destroyed, and three out of four of the small bones of the tympanum be removed, an almost total deafness ensues; but the ear, after a time, begins to recover its powers, and, in the end, regains them, with that degree of imperfection only, which, in my former Paper, I have described in the case of Mr. P———.† The following fact appears to confirm the truth of this statement. Mr. Radford, surgeon, of Newington Butts, informs me, that in the year 1779, he attended a woman who had an ulcer in the throat, by which a portion of the palate was destroyed, and the tonsils and Eustachian tube so much injured, that in the attempt to swallow, a part of the liquid ran through her ears; yet, notwithstanding these ravages, she neither complained of any defect in her hearing, nor had the slightest appearance of deafness. In cases, however, where the discharge of matter which pro-

* It was formerly supposed, that there was naturally a communication between the external ear and the throat, through the membrana tympani; an opinion which it is now almost unnecessary to say is without foundation.

† Vide Philosophical Transactions for 1800, page 152.

duced the destruction of the membrane continues, should a fungus arise on the periosteum of the tympanum, or exfoliation of the bones forming this cavity occur, and more especially should the stapes separate, very considerable deafness will be the consequence.

When the membrane of one ear only is destroyed, a greater degree of deafness takes place in that ear, than would happen in either, were the membrane destroyed in both. This, as I stated in my former Paper, probably arises from the disuse into which the imperfect ear falls, from its being less quick in its powers than the other; a conjecture which seems to be verified by the following fact.

Mr. G——, a merchant in the city, lost, at an early period of life, so great a portion of the membrana tympani of the left ear, that no more of it remained than appears in Fig. 3; and, as he heard somewhat better with his right ear than with his left, he was little in the habit of employing the latter, and considered himself at length as almost totally deaf in it. Becoming, however, in the month of December last, deaf in the right ear, and being obliged, in consequence, to employ the other, he found that the left ear was by no means deprived of its powers; although he could force air from his mouth through that ear, and, if he suddenly thrust his finger into the meatus, the air was heard to rush through his nostrils.

I feel a hope that the foregoing observations will tend to something more than merely to gratify curiosity, and will be productive, in the end, of lasting benefit; for they have induced me, in one species of deafness, to try the effect of an operation, which has, in several instances, proved successful.

The deafness to which I allude, is that which arises from an

obstruction of the Eustachian tube; and the operation consists in puncturing the membrana tympani.

The tympanum of the ear is formed like a drum; and, as a drum will produce very little sound, unless air be admitted by a hole in its side, so, in the usual state of the ear, the membrana tympani cannot perform its office, if air has not free access to the cavity of the tympanum. The air, thus essential to hearing, passes from the throat to the ear by the Eustachian tube; so that the membrana tympani is placed between two portions of air, the one contained in the meatus, the other in the cavity of the tympanum. Accordingly, if the Eustachian tube becomes obstructed, the air confined in the tympanum being unable to yield, the membrana tympani must cease to vibrate; and thus, sound being no longer conveyed to the interior parts of the organ, a permanent deafness must ensue.

There are several causes by which a closure of the Eustachian tube may be produced.

It may arise, first, from a common cold affecting the parts contiguous to the orifices of the tube, and thereby preventing the free passage of air into the tympanum. The deafness thus produced, however, is often merely temporary. But the frequent recurrence of such attacks may produce permanent enlargement of the tonsils, which, by their pressure on the Eustachian tubes, will occasion a permanent deafness.

In February last, an instance occurred, of a person who had thus been rendered deaf since the year 1793; and I have met with another instance of deafness from a similar cause.

Secondly, The scarlet fever occasions ulcers in the throat, which, in healing, frequently close the Eustachian tubes, thereby producing lasting deafness.

As this fever occurs particularly in young persons, who are but little subject to a defective state of the nerves of the ear, the greater hope of relief may be entertained from the operation already mentioned.

Thirdly, A venereal ulcer in the fauces, by the cicatrix it produces, often occasions a closure of the Eustachian tube, causing a deafness which nothing but the operation here spoken of can relieve.

Fourthly, I have known this closure of the tube produced by an extravasation of blood in the cavity of the tympanum.

Lastly, I have seen one instance of a stricture in the tube, which, although it did not entirely obstruct the passage of the air, yet rendered it extremely difficult. To enable himself to hear, the gentleman who was the subject of this disease, was under the necessity of forcing air, from the mouth, into the cavity of the tympanum, which pushed the membrana tympani towards the meatus; then, pressing gently upon the ear, he forced out a part of the air which the tympanum contained; thus giving the membrane liberty to vibrate, and producing an immediate increase in the power of hearing.

The above mentioned are the most common causes of the closure of the Eustachian tube; and I have reason to think, from the experience I have already had, that they may all be remedied by puncturing the membrana tympani.

I was led to this operation by reflecting that, as an aperture in this membrane did not appear to injure the power of the ear, and a small opening would be sufficient to admit a free passage of air to and from the tympanum, perhaps a substitute might be thus easily found for the Eustachian tube, and the membrane, by such an aperture, be restored to its natural functions. Oppor-

tunities were soon afforded me of trying the effects of this operation, and of putting my idea to the test of experiment. Of the instances by which it has been verified, the following appear to me most worthy of selection and record.

Case 1. A woman about thirty-six years of age consulted me, in December last, respecting some disorder in her child. In attempting to converse with her, I found her so extremely deaf that it was with difficulty I could make her hear me. Questioning her upon the subject of her deafness, she informed me that she had been thus afflicted since the year 1793; and I found that it had arisen from the tonsil glands becoming enlarged by a cold, which she caught in the winter of that year. As she was anxious to be relieved, I immediately punctured the membrane of the left ear, being that in which the hearing was most defective. The operation was no sooner performed, than, to my great joy, and of course to hers, I found that, in that ear, she could hear what I said to her, without any particular exertion on my part to speak loud. She staid with me about half an hour; and, when she left me, was capable of hearing every thing that was said in the ordinary tone of conversation.

Case 2. Ann Daley was admitted under my care, in Guy's Hospital, on the 21st of January, 1801. She was so deaf that, unless words were spoken close to her ear, it was impossible to make her hear them. She had been thus far deprived of hearing for the space of six weeks; and the deafness had been occasioned by some ulcers which had existed in the fauces. On the 25th of January, four days after her admission into the hospital, I punctured the membrana tympani of the left ear; having previously taken care (the better to ascertain the effects of the operation) to hold a watch to the ear of the patient, the beating of

which she could not distinguish, unless it was pressed against her head. After the operation, I instantly repeated that experiment, and found that, with the ear I had punctured, she could distinctly hear the watch, though it was held at the distance of several feet; whereas, with the opposite ear, she was still unable to hear it beat, unless, as before, it was pressed against her head. Mr. STOCKER, apothecary to the hospital, witnessed the effects of this operation.

On the 28th of the same month, I performed the same operation on her right ear, in the presence of several medical gentlemen, who satisfied themselves as to the cause and degree of her deafness; the ear upon which I first operated having been purposely closed. As soon as the puncture was made, the trial with the watch was again resorted to; and she could hear it beat at the same distance as with the other ear; and could hear us speak, in the common tone of voice, as distinctly as we could hear one another.

To ascertain with certainty whether she really heard the beating of the watch, I placed it at a considerable distance from her, and asked her if she still heard it. To which she answered, "Yes, perfectly." I then stopped the watch, without her knowing it; and, the question being repeated, she listened for a while, then said, "I must have been deceived, I do not hear it:" but, the moment I set it again in motion, she called out, "I hear it "now, and as well as I ever did in my life." In this state her hearing continues; the deafness having never, at any time, returned.

The cause of this deafness was obviously in the throat. The disease had not existed sufficiently long to produce any other derangement in the ear; and the good effect of the operation

was therefore so immediately apparent, that it could not be doubted by the most sceptical observer.

CASE 3. Mr. ROUND, of Colchester, consulted Dr. BAILLIE respecting his son, Mr. JOHN ROUND, aged 17, who had laboured, from his birth, under such a degree of deafness as would have incapacitated him from engaging in business. Dr. BAILLIE, having satisfied himself that there was no nervous defect in the ear, referred him to me. I found that this gentleman had been born with an imperfect state of the fauces, which rendered him incapable of blowing his nose; that the Eustachian tubes had no openings into the throat, and, therefore, that he was unable to force air from the mouth into the ear. The auditory nerves, however, were perfect; for he could distinctly hear the beating of a watch, if placed between the teeth, or against the side of the head; and he never had perceived any buzzing noise in his ears. I therefore advised him to submit to the operation of perforating the membrana tympani; to which he cheerfully consented. The moment this was done, a new world was opened to him; and the confusion produced by the number of sounds which immediately struck his ear, made him sink upon a chair, almost in a fainting state. From this state he recovered in about two minutes; and, finding that his hearing was completely restored upon the one side, he wished the operation to be performed upon the other; which was immediately done, with the same happy result, and without his experiencing the same confused sensation as before.

Near two months after the operation, I had the pleasure to receive an assurance from him, that he had suffered no relapse, nor any inconvenience from the opening which I had made, and that his hearing continued perfect.

Case 4. Mr. Brandon, of Upper Clapton, sent a person to me in January last, who had received a blow upon his head, which had occasioned symptoms of concussion of the brain, and was attended with a discharge of blood from each ear. From the effects which the blow had occasioned on the brain, he speedily recovered; but the deafness, which had immediately followed from the accident, continued. I cleared the meatus from the blood it contained, without any relief being derived to the patient; and, suspecting that a quantity of blood was lodged in the tympanum, and the vibration of the membrane thus prevented, I some days after punctured the membrana tympani. Upon withdrawing the instrument, some dark-coloured blood appeared on its point; and, whenever I examined his ear afterwards, there was the same appearance of blood mixed with the wax of the ear, which continued to discharge for about ten days after the operation, during which period the hearing was gradually restored. I have formerly known instances of permanent deafness from this cause; and I think it not improbable that the blood thus effused has become organised, and continued to fill the cavity of the tympanum.

The operation to remedy the species of deafness here described, consists in passing into the ear a canula, of the size of a common probe, in which a trocar is concealed; the canula is to rest upon the membrana tympani, and the trocar is then to be thrust through the membrane.

The trocar should be so adjusted as not to pass more than $\frac{1}{8}$ of an inch beyond the canula, to prevent its reaching the opposite side of the cavity of the tympanum. Should it however touch the periosteum of the tympanum, it can be productive of no serious harm. The aperture should be made in the anterior

and inferior part of the membrane, under the manubrium of the malleus, which must not be injured in the operation; and it is therefore necessary that the operator be acquainted with its exact situation.

Though the membrana tympani be vascular, the vessels are so small that they bleed but little; and therefore, if much blood is discharged, the operation cannot have been properly performed.

In an ear otherwise healthy, the operation is attended with so slight a degree of pain, that when it has been performed in one ear, the patient expresses no unwillingness to submitting to it in the other. The sensation which it occasions is momentary; and no subsequent inconvenience of any kind arises.*

As this operation will not afford relief in any cases of deafness, except such as arise from a closed Eustachian tube, I am anxious that it should be performed in those only which are clearly of that description. The criteria by which I judge whether the tube is closed or open are the following.

First, If the person in whom it is suspected to be closed, should feel, in blowing the nose violently, a swelling in the ear, from the membrane being at that time forced outward, the tube is open; for, when closed, no such sensation is produced.

Secondly, The Eustachian tube may be closed, yet the beating of a watch may be heard, if it be placed between the teeth, or pressed against the side of the head; and, if it cannot be heard when it rests upon the teeth, this operation cannot relieve, as the power of the auditory nerves must have been destroyed.

* If the ear has been previously irritated by stimulating applications to the meatus, the operation will then be painful; it is therefore proper to wait until the inflammation has subsided.

Thirdly, It is right to inquire if the deafness was immediately preceded by any complaint in the throat.

Lastly, In a closed Eustachian tube there is no noise in the head, like that which is hereafter described as accompanying nervous deafness.

The causes of deafness are extremely numerous; and many of those which affect only the meatus auditorius, the membrana tympani, the cavity of the tympanum, and the Eustachian tube, admit of relief from surgical assistance.

But there is one species of deafness in which, as it depends, like the gutta serena of the eye, upon an affection of the nerve, it would be as absurd to expect relief could be derived from any operation upon the membrana tympani, as it would to suppose that a person diseased in the optic nerve could be restored to sight by extracting the cataract. This species of deafness occurs more frequently than any other, happening generally in old persons; but sometimes also, in the delicate and irritable, in the earlier stages of life; I have known it produced by anxiety and distress of mind. Its approach is generally gradual: the person hears better at one time than at another; a cloudy day, a warm room, agitated spirits, or the operation of fear, produce a considerable diminution in the powers of the organ. In the open air, the hearing is better than in a confined situation; in a noisy, than in a quiet society; in a coach when it is in motion, than when it is still. A pulsation is often felt in the ear; a noise, resembling sometimes the roaring of the sea, and at others the ringing of distant bells, is heard.

This deafness generally begins in a diminished secretion of the wax of the ear, which the patient attributes to some unusual exposure of the head to cold; and this continues so long as the

disease remains. In the commencement of this complaint, it may be cured by the application of such stimulants as are capable of exciting a discharge from the ceruminous glands; which stimulants ought to be introduced into the meatus, for that purpose. If these are used so as to irritate, without exciting a discharge, they are rather prejudicial than otherwise. But, if the organ has been long neglected, and the disease has been suffered to make considerable progress, I believe that no hope of cure can be rationally entertained.*

There is another cause of deafness, to which I fear no art of the surgeon can apply a remedy; this is, an alteration of the contents of the labyrinth. The interior part of the ear, called the labyrinth, is naturally filled with water, upon which the auditory nerve is expanded; and it is by the undulations of this fluid, that impressions are made upon the nerve, and conveyed to the brain.

If a solid substance be generated in this part of the ear, instead of the fluid, the powers of hearing will be destroyed, or at least very considerably impaired. From the following dissection, this would appear to be at least one cause of deafness in those who are born with this infirmity, and who are also dumb, unless assisted by particular instructions.

Mr. Cline, being requested by Dr. Walshman, of Kennington, to examine the head of a young man who had died of a fever, and who had been born deaf, and was consequently dumb, found, upon dissecting the organs of hearing, all the parts perfectly formed, and as usual in a healthy ear, except

* I have, in several cases of this kind, made trial of the operation of opening the membrana tympani, without finding that it afforded any other relief than that of diminishing the noise in the head, which always accompanies it.

the vestibule, cochlea, and semicircular canals; these were filled with a substance of the consistence of cheese, instead of the fluid which they usually contain. From a defect like this, deafness could not fail to arise; for, as the substance occupying the place of the watery fluid could not be made to undulate by the motions of the membrane of the fenestra ovalis and rotunda, all impressions upon the auditory nerve were completely prevented.

I have thought it right to describe the foregoing instances of deafness, because they are liable to be confounded with that which arises from a closed Eustachian tube. Others might perhaps have been added; but various professional engagements have prevented me from devoting so much time to this subject as I am confident it merits. I have, however, the pleasure to reflect, that several individuals have been restored to society, who were before almost incapacitated from its enjoyments. I hope others will be induced, by this success, to second my feeble efforts, and to direct their attention to a subject which appears to be of the highest importance, and to have been too much neglected by medical men; for a knowledge of the structure of the ear is by no means general in the profession, and still less are its diseases understood. A prejudice has prevailed, that the ear is too delicate an organ to be operated upon, or, as it is commonly exprest, *tampered with;* and thousands have thus remained deaf for the rest of their lives, who might have been restored to hearing, had proper assistance been *early* applied. But this prejudice, it is hoped, will now be done away; since it appears, that the part which has been thought most essential to hearing, viz. the membrana tympani, may be injured by disease, or may be broken by violence, without a deprivation

of the sense of hearing, and that, even when this membrane is entirely destroyed, another is found to perform its functions; so that the powers of the organ have still been, in a considerable degree, preserved.

Let it also be recollected, as a farther encouragement, that in the operation I have mentioned, little pain is felt, no dangerous consequences follow, and, even if it is sometimes performed unsuccessfully, the patient is left with the same capacity as before, of receiving relief from other remedies.

Explanation of the Figures. See Plate XXXIII.

Fig. 1. Shews the external ear, the meatus auditorius, membrana tympani, and Eustachian tube.

A, The meatus.

B, The membrana tympani.

C, The cavity of the tympanum.

D, The Eustachian tube.

Fig. 2. Shews the perforating instrument, as it is introduced in the operation.

Fig. 3. The membrana tympani of Mr. G——, of which only that part which appears of a lighter colour remains.

Fig. 4. The membrane lacerated by a blow.

Fig. 5. The membrane lacerated in an attempt to extract a pin.

Fig. 6. Shews the membrana tympani of a medical man in the city, having a fungus projecting through it: in this ear, he is considerably deaf.

Fig. 7. The other membrane of the same gentleman.

Fig. 8. One of the membranes of Mr. P——, whose case I described in my former Paper.

Fig. 9. A membrana tympani in its natural state, shewing the attachment of the manubrium of the malleus.

Fig. 10. The appearance of the membrane after having been punctured.

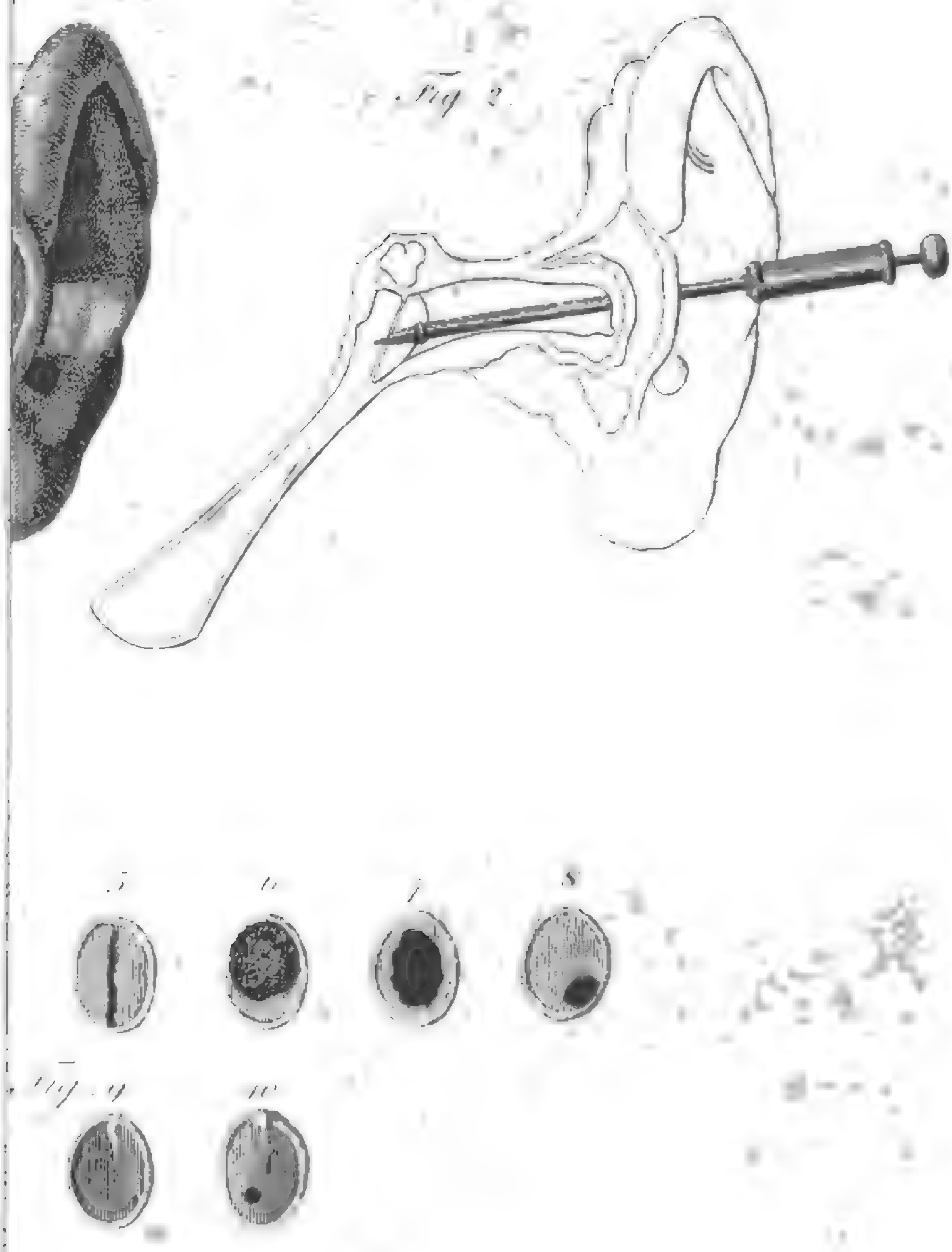
Fig. 2.
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Fig. 9
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	An Inquiry into the Nature of that Swelling in the lower Extremities, which sometimes happens to lying-in Women. Part 2. Manchester, 1801. 8°	Mr. Charles White, F. R. S.
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	Travels in Portugal, by H. F. Link, Translated by J. Hinckley. London, 1801. 8°	John Hinckley, Esq.

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